The Dissertation Committee for Kai Fung Law Certifies that this is the approved version of the following dissertation:

Early Career Experience and Optimism Spillover

Committee:

Laura T. Starks, Supervisor

Michael B. Clement

Bing Han

Alok Kumar

Clemens Sialm

Early Career Experience and Optimism Spillover

by

Kai Fung Law, B.Sc.Hons.; M.S.; M.S.Fin

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Dedication

To my parents

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Early Career Experience and Optimism Spillover

Kai Fung Law, PhD

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Supervisor: Laura T. Starks

Using a long panel on employment history, I exploit a novel setting to examine if sell-side analysts carry over their early experience into their future professional careers. I find that analysts' early mentorship experience has a long-lasting impact on their professional styles. Analysts are more optimistic if they work with optimistic mentors in their first jobs as junior analysts: they issue more strong buy recommendations and upgrade jumps, and they are also more optimistic in earnings forecasts and price targets. While it is easy to pick up their mentors' styles, I show that it is apparently harder for them to learn their mentors' skills, as indicated by the lack of spillover in forecast accuracy. Only talented superstar mentors can unwind this pattern, passing their skills and reputation to their protégés. The market — especially sophisticated institutional investors — is smart in identifying the apprentices of optimistic mentors as short-run market reactions to their forecast revisions are weaker. Collectively, these results have important implications for financial economists and regulators (on a new source of optimism), for analyst profession (on talent management and portability), and for market participants (on information dissemination).

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CHAPTER 1: INTRODUCTION

Do individuals' past experiences affect their future economic behaviors? While traditional economic and finance theories assert that individuals are rational, an increasing number of economic and finance studies have shown that individuals' past experiences have a long-lasting impact on their future personal, or even professional, decision making.

This dissertation uses the sell-side analyst profession as *a* setting to answer whether individuals carry their past experiences into their future decision making. More specifically, the following chapters attempt to shed light on the extent to which analysts' styles and skills are influenced by their early mentorship and labor market experience when they first start their analyst jobs.

Following the literature review in Chapter 2, Chapter 3 first examines whether sell-side analysts' optimism and skills are influenced by their early career mentors, which are defined as those colleagues who work in the same brokerage house (i.e., same house), cover the same sector (i.e., same team), and have more years of professional experience (i.e., veteran) during analysts' early careers as junior analysts. The major findings are summarized as follows. First, analysts are more optimistic (pessimistic) in their future professional careers if they work with optimistic (pessimistic) mentors in their early careers. More specifically, analysts with optimistic mentors issue more strong buy recommendations and upgrade jumps, and are more optimistic in earnings forecasts and price targets.

While there is a spillover in optimism, there is no spillover in accuracy. In other words, while it is easy for analysts to pick up their mentors' styles, it is apparently harder for them to pick up their mentors' skills. Only talented superstar mentors (i.e., all-star analysts) can break this pattern, spilling their forecasting skills to their protégés. What is more intriguing is that, controlling for accuracy, optimism, brokerage size, analyst characteristics (i.e., general experience, firm-specific experience), or portfolio complexity (i.e., number of companies, number of industries), analysts with all-star mentors in their early careers are almost 6% more likely to be elected as all-star analysts than analysts with no star mentors.

Chapter 4 broadens the analysis to ask the following questions: does the market care about the mentors of optimistic analysts? And is every market participant able to take this mentor-mentee relationship into account when digesting analysts' forecast revisions? The results indicate that the market generally responds more weakly to the revisions made by analysts who have optimistic mentors. More specifically, the evidence shows that sophisticated institution investors substantially discount the revisions made by the protégés of optimistic mentors, regardless of the direction of revisions. In short, the market is smart enough to identify the analysts of extremely optimistic mentors and discounts their revisions.

Finally, using the cyclical labor market as a form of exogenous shock, in Chapter 5 I exploit a novel setting to examine if sell-side analysts differ in their style and skills depending on their early career macroeconomic environment. This paper shows that analysts who start their careers in a tough labor market are more pessimistic, less accurate, and less likely to become all-star analysts than their non-recession peers. While their forecast accuracy improves as they gain more experience, they continue to hold onto their pessimism. On the contrary, while boom analysts are more optimistic, they are not necessarily more accurate. Moreover, their optimism is shown to be short-lived. Collectively, the above evidence suggests that the development of firm-specific human capital is endogenous to time-varying macroeconomic conditions. The asymmetric performance of boom and recession analysts indicates that there are unobserved heterogeneities in their human capital and information dissemination.

CHAPTER 2: A CURIOUS CASE OF MENTOR INFLUENCE, ANECDOTAL EVIDENCE, AND LITERATURE REVIEW

Do sell-side analysts carry their early mentorship experience into their future careers? This chapter motivates this research question by looking into the reaction of various analysts to the recent BP deepwater oil spill. This chapter then provides anecdotal evidence on analyst mentorship on Wall Street, followed by a literature review.

2.1 A Curious Case of Mentor Influence

On April 20, 2010, BP's deepwater oil platform in the Gulf of Mexico exploded, killing or injuring 28 workers. The tragic explosion was followed by a deepwater oil spill. As the leak was at a depth of more than 5,000 feet on the seabed, BP failed to contain the leak. During the following three months up to July 15, 2010, more than 53,000 barrels of crude oil leaked from the damaged gusher into the wide open sea. That spill is the worst in the U.S. history.[1] In view of numerous litigations following the accident, BP in 2010 set up a $20 billion compensation fund and estimated the total cost of cleaning, criminal or civil fines, and liability compensation at $41 billion.[2]

The catastrophic impact of the oil spill was immediately reflected in BP's stock price. Figure 2.1 shows the movements of BP's stock price from January to December 2010, a period surrounding the deepwater oil spill accident. After news of the oil platform explosion was aired in the evening of April 20, BP's stock price responded by slipping 0.64% to $60.09 on the next trading day. When more details of the spill surfaced on April

[1] For instance, ABC News (May 27, 2010), "BP oil spill called worst in U.S. history, as MMS official steps down", The Telegraph (May 29, 2010), "BP disaster: worst oil spill in US history turns seas into a dead zone", and USA Today (July 1, 2010), "BP oil spill hits record as Gulf's worst".
[2] Reuters (June 30, 2010), "Gulf oil spill claims should work faster".

26-27, the market finally realized the massive scale of the damage and the economic implications of the catastrophe. In the next two trading days, the share price further plunged by about 5%. While there were some subsequent rebounds in the following months, BP's share price continued to tank, reaching its lowest of $27.02 on June 25. That drop represents a 55% loss of share price after the accident in which more than half of BP's market capitalization vaporized in two short months.

While investors panicked and continued to vote with their feet with fire sales, Wall Street analysts were calm and puzzlingly optimistic. As of May 2010, none of 34 analysts who follow BP had issued a single "Sell" or "Underperform" rating in view of the accident. In fact, one of the analyst reports did not even mention the accident.[3] As of May 20, one month after the explosion, the mean recommendation across Wall Street analysts covering BP was 2.36,[4] a recommendation level equivalent to a "Buy", and an increase from a consensus of 2.27 a month before on April 15 before the deepwater oil spill.[5] Perhaps more surprising is a string of upgrade announcements by various analysts, either to "Strong Buy" or "Buy", shortly after the accident.[6] An often-cited argument supporting the upgrade decision is that the market over-reacted and that the plunge is not warranted with respect to the temporary shock. Such optimistic biases are perhaps not surprising in view of decades of financial economics research on analyst incentives and career concerns.

Now turn to a seemingly unrelated question: could analysts' early career experience explain their different reactions in this oil spill accident? For Wall Street

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[3] Reuters (June 18, 2010), "Special report: Amid the Gulf Crisis, Wall St touted BP stock".
[4] 1 is "Strong Buy", whereas 5 is a "Strong Sell".
[5] The consensus recommendation data were obtained from IBES in WRDS in July 2011.
[6] The Wall Street Journal (May 28, 2010), "Large stock focus: Intel, Microsoft, BP and AmEx all jump"; The Washington Post (June 6, 2010), "BP investors struggle to factor in the unfathomable"; The Wall Street Journal (July 7, 2010), "Shares get boost from below".

analysts, the first and foremost memorable experience is their mentorship experience when they first started their jobs as junior analysts. As protégés in an apprenticeship system, analysts learn their craftsmanship from writing research reports to building earnings models by interacting with experienced seniors. These mentors are veteran analysts with years of professional experience, covering the same industry, and presumably serving as role models for career advancements. If analysts worked with a group of optimistic mentors early in their careers, they could carry their experience over in their future professional lives.

Figure 2.2 suggests this possibility. Going back to the BP example, I sort all analysts covering BP at the time of the accident into two groups: a "Strong Buy" group who upgraded, reiterated, or initiated a "Strong buy" recommendation after the deepwater oil spill; and a "Non Strong Buy" group who did otherwise. I track their mentors and compute their historical optimism in recommendations and earnings forecasts during the analysts' early careers, where early career is defined as either an analyst's first job or the first three years of analyst's first job, whichever is shorter. I then rescale all optimism measures from 1 (least optimistic) to 10 (most optimistic) for comparison. Figure 2.2 shows that the mentors of "Strong Buy" analysts were both more optimistic than "Non-Strong Buy" mentors when analysts first started their professions. "Strong Buy" mentors were 50% more optimistic in earnings forecasts than, and twice as optimistic as, "Non-Strong Buy" mentors. This suggests the possibility that analyst optimism can be influenced by mentor optimism in their early careers. While this example is based on a subset of analysts and on a single catastrophic accident, it illustrates the simple idea behind this study: analysts carry their early career experience over in their future professional activities.

2.2 Anecdotal Evidence: Analyst Mentorship on Wall Street

Often after attending weeks of intensive orientation and training programs, a newly-hired junior analyst would be assigned to one or more mentors, who are experienced analysts with years of industry experience and assume the main role of providing on-the-job training:

> The conventional mechanism for orientation and training of beginning analysts was the position of junior analyst. Most research departments offered very little further organized training or mentoring, relying nearly exclusively on junior-senior interaction to teach the basic intellectual and marketing skills. *(Groysberg, 2010, p.203)*

Some of the largest wall-street brokerage houses take mentorship experience seriously. For instance, Bernstein requires their veteran analysts to devote fully 20% of their time to mentoring, hiring and other institution-building activities (Groysberg, 2010 p.218). Other investment banks such as Goldman Sachs and Morgan Stanley also offer multiple structured or informal “mentor-and-buddy” programs to help junior analysts to develop their skills and networks through social interaction with experienced analysts. It is also not uncommon that experienced analysts work hand-in-hand with junior analysts on various projects, opening the on-the-job doors to juniors so that they learn how to write their research reports, run earnings model to forecast firms’ future growth, or pitch their ideas to sales force or institutional investors. For instance,

> In early June, I asked Megan Kulick and Mark Kastan, who had been hired to replace… to run a complex series of financial models assuming the Baby Bells started to offer long distance services… We worked on the models for six solid weeks *(Reingold and Reingold, 2007 p.85)*

While junior analysts usually start with number crunching, they tend to accept more responsibilities such as dealing with clients or sales force, attending clients' meetings or writing research reports along their career path. Apart from providing pedagogical guidance, mentors also serve as role models for junior analysts in the apprentice system:

> I didn't see anyone daring put the squeeze on Ed, my mentor. He managed to have strong opinions – positive and negative – without being pressured by anyone, not bankers, not management. As his protégés, I felt insulated and protected *(Reingold and Reingold, 2007 p.37)*

or networking instrument for exposure and visibility:

> In addition to the summer associates, these nights out generally included as many fulltime associates as our mentors were able to round up and… *(Rolfe and Troob, 2001, p.54)*

The above anecdotal evidence suggests that mentorship programs are uncommon among sell-side equity analysts since the heavy reliance on mentor-apprentice model is a unique feature of equity analyst industry.[7]

2.3 Literature Review

This dissertation is motivated by the literature on analyst optimism, social interaction, and the emerging literature on the impact of personal past experience on future behavior. I provide a brief summary of these strands of literature in the following sections and summarize the main testable hypotheses.

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[7] While the above provides anecdotal evidence on various mentorship programs on the Wall Street, the mentors in this dissertation do not require neither a formal mentorship in any brokerage house nor a formal one-to-one reporting relationship between mentor and mentees.

2.3.1 Analyst Optimism and Career Concerns

The first strand of research that motivates this dissertation is the analyst literature on forecast optimism. A common finding in analyst literature is that analysts tend to issue optimistic forecasts that are biased upward than actual earnings (Brown, Foster and Noreen 1985, Stickel 1990, Abarbanell 1991, Butler and Lang 1991, Dreman and Berry 1995, Chorpa 1998).

Previous literature has documented a number of external or strategic factors explaining this optimism bias. For instance, analysts are more optimistic for stocks that have equity/bond underwriting relation (Francis and Philbrick 1993, Dugar and Nathan 1995, Lin and McNichols 1998, Michaely and Womack 1999, Dechow, Hutton and Sloan 2000, etc.) or that have greater ownership by affiliated mutual fund families (Mola and Guidolin 2009), that are managed by affiliated dealers (Chung and Cho 2005)., that they hold favorable information (McNichols and O'Brien 1997, Hayes 1998, Das, Guo, and Zhang 2006).

Analysts also exhibit optimism to generate commission for employing brokerages, to promote specific stocks (Hayes 1998, Irvine 2000, Irvine 2004, Jackson 2005, Johnson and Schwartz 2005, Cowen, Groysberg and Healy 2006), to curry favor to firm management in exchange for better information access (Francis and Philbrick 1993, Das, Levine, and Sivaramakrishnan 1998, Irvine 2004, Richardson, Teoh, and Wysocki 2004, Chen and Matsumoto 2006, Ke and Yu 2006, Ertimur, Zhang, and Muslu 2010). Analysts are also shown to be less optimistic when the level of institutional ownership is high for their covered stocks, suggesting institutional ownership to serve as a watchdog (Ljungqvist, Marston, Starks, Wei, and Hong 2007).

Studies suggest that analysts are more optimistic because of career concerns. For instance, Hong and Kubik (2003) show that, controlling for accuracy, analysts who are more optimistic relative to their peers are more likely to be promoted to prestigious brokerage houses. Ke and Yu (2006) find that analysts who are initially optimistic and subsequent pessimistic during firm's fiscal-year are less likely to get fired.

2.3.2 Social Interaction

The literature on economics has extensively documented the influence of social interaction and social ties in the context of various social issues.[8] In a related vein, an emerging strand of finance papers analyzing social connectedness suggests that personal social ties have a strong influence in professional domains.[9] Hong, Kubik, and Stein (2005) document a word-of-mouth effect in which mutual fund managers herd into buying or selling the same stocks held by other mutual fund managers in the same city. Cohen, Frazzini, and Malloy (2010b) find that analysts who have educational ties with senior firm officers generate more accurate earnings forecasts and their recommendations significantly outperform, thereby providing further support that the "old boy" network commands a positive premium. Using firm directors' death and retirement as an identification instrument, Fracassi and Tate (2011) show that the external network ties between CEOs and board directors weaken board monitoring. The basic findings of all these studies suggest that social interaction and social ties have a strong and economically vital influence on personal or professional decision-making.

[8] For example, education: Aaronson (1998), Foster (2006); Carrell, Fullerton, and West (2009); crime: Glaeser, Sacerdote, and Scheinkman (1996); cigarette smoking: Powell, Tauras, and Ross (2005); ethnicity and neighborhoods: Borjas (1995); teenage pregnancy and high school dropout: Evans, Oates, and Schwab (1992), Gaviria and Raphael (2001), etc.

[9] For instance, Duflo and Saez (2002), Hong and Kubik (2004), Massa and Simonov (2005), Hong, Kubik, and Stein (2005), Hochberg, Ljungqvist, and Lu (2007), Horton and Serafeim (2009), Cohen, Frazzini, and Malloy (2010a), Cohen, Frazzini, and Malloy (2010b), Engelberg, Gao, and Parsons (2010), Fracassi and Tate (2010), Gray and Kern (2011), Hwang and Kim (2011), Cohen and Malloy (2011), Lerner and Malmendier (2011), etc.

2.3.3 Impact of Past Experience

A slowing growing strand of literature also shows that individuals' past experience has a long-lasting impact on their future economic behavior. Malmendier and Nagel (2011) show that individuals who experienced the Great Depression are less willing to take financial risk. Kaustia and Knüpfer (2008) find that the past IPO experience of investors affects their future subscription decisions. Malmendier, Tate, and Yan (2011) also show that CEOs with a military background pursue more aggressive corporate leverage strategies. Other recent finance studies also show that past experience affects corporate financial policies (Graham and Narasimhan, 2004; Schoar, 2007), subscriptions to initial public offerings (Chiang, Hirshleifer, Qian, and Sherman, 2011), and the investment strategies of mutual fund managers (Greenwood and Nagel, 2009). Prior management literature also shows that mentorship has a significant impact on protégés' turnover decisions. Whitely and Coetsier (1993) and Whitely, Dougherty, and Dreher (1991) both find that managers who participate in mentoring programs during their early careers have better promotion rates. Payne and Huffman (2005) show that mentoring programs reduce the turnover rate of U.S. Army officers. The common finding of these studies is that an individual's past experience has a strong and long-lasting impact on the individual's future economic behavior.

No study so far has investigated whether the past experience of sell-side financial analysts affects their professional activities. Prior studies document that analysts are more accurate if their firm-specific coverage experience increases.[10] Clement, Koonce, and Lopez (2007) find that veteran analysts who have rich task-specific experience make

[10] For example, Clement (1997), Mikhail, Walther, and Willis (1997), McNichols and O'Brien (1997), and Jacob, Lys, and Neale (1999).

more accurate earnings forecasts, providing evidence that analysts learn from their past experience. The closest study to this chapter is the study by Antoniou (2010), who shows that analysts are affected by colleagues of current employing brokerage houses. The primary difference between our studies is that Antoniou (2010) asks the question to what extent an analyst's optimism is affected by his or her colleagues whom the analysts interact with in a contemporaneous manner, whereas this study investigates the influence of his or her mentors, defined as senior experienced colleagues during early careers, with whom the analysts worked with in a historical context. More specifically, my study exploits a unique feature of the analyst apprenticeship system and asks a broader question: how does past mentorship experience shape analysts' future activities in a professional domain.

CHAPTER 3: EARLY CAREER EXPERIENCE AND OPTIMISM SPILLOVER

3.1 Introduction

While there is a widespread agreement that sell-side equity analysts are optimistic for various strategic reasons,[11] little is known about how their early mentorship experience influences their professional optimism. A small yet growing collection of studies shows that past personal experience affects future economic behaviors: for instance, the Great Depression babies are less willing to take financial risks (Malmendier and Nagel (2011a)); investors' experience of personal gains increases the likelihood of future IPO subscription (Kaustia and Knüpfer (2008)); and CEOs with military backgrounds pursue more aggressive corporate leverage strategies (Malmendier, Tate, and Yan (2011)). The idea that economic agents assign a non-zero weight on past personal experience when updating their beliefs is consistent with social learning in economics literature (Banerjee (1992), Ellison and Fudenberg (1993, 1995), Bala and Goyal (1998)); however, no study has examined the influence of analysts' past experience on their professional careers. Even though we have the largest number of equity analysts in the world, we still do not fully understand how analysts' optimism is

[11] A common finding in the analyst literature is that analysts tend to issue optimistic forecasts that are biased upward compared to actual earnings (Brown, Foster, and Noreen (1985), Stickel (1990), Abarbanell (1991), Butler and Lang (1991), Francis and Philbrick (1993), Dreman and Berry (1995), Hunton and McEwen (1997), Chorpa (1998)). Specifically, analysts tend to walk down their forecasts during firms' fiscal years (Richardson, Teoh, and Wysocki (2004). There are several strategic reasons to explain analyst optimism: client affiliation or trading commission incentives (Francis and Philbrick (1993), Dugar and Nathan (1995), Hayes (1998), Lin and McNichols (1998), Michaely and Womack (1999), Dechow, Hutton, and Sloan (2000), Irvine (2000, 2004), Chung and Cho (2005), Jackson (2005), Johnson and Schwartz (2005), O'Brien, McNichols, and Lin (2005), Cowen, Groysberg, and Healy (2006), Mola and Guidolin (2009)), career concerns (Hong, Kubik, and Solomon (2000), Hong and Kubik (2003)), or better information access (Francis and Philbrick (1993), Das, Levine, and Sivaramakrishnan (1998), Lim (2001), Irvine (2004), Richardson, Teoh, and Wysocki (2004), Chen and Matsumoto (2006), Ke and Yu (2006), Ertimur, Muslu, and Zhang (2011)). In contrast, institutional investors serve as a watchdog, dampening the excessive optimism (Ljungqvist, Marston, Starks, Wei, and Hong (2007)).

influenced by their on-the-job learning experience. As analysts are often blamed for their optimistic view,[12] understanding a source of analyst optimism is the key to improving our knowledge about how equity analysts process and disseminate information.

In this chapter, I examine whether analysts' early career experience influences their future professional styles. Specifically, this study asks whether sell-side analysts differ in their professional optimism depending on the styles of the mentors with whom they work in their first jobs as junior analysts. The key conjecture of this mentorship experience hypothesis is that analyst optimism should exhibit a systematic relationship to the optimism of the mentors with whom a protégé works in his or her early career, which is defined as the analyst's first analyst job or the first three years of the analyst's first job, whichever is shorter. Analysts are expected to exhibit more optimistic in their later careers if they work with optimistic mentors in their early careers. If mentors in analysts' early careers have no influence on their protégés' professional styles, we should not observe any systematic relationship between analyst and mentor optimism.

A few simple bar charts in Figure 3.1 suggest this systematic pattern in the raw data. Using a host of optimism metrics that comprehensively describe analysts' professional styles: (i) portfolio weight of strong buy recommendations (*Strong Buys*), (ii) frequency of upgrade jumps (*Upgrade Jumps*), (iii) optimism in earnings forecasts (*Earnings Forecast Optimism*), and (iv) optimism in price targets (*Price Growth Optimism*), the patterns in the figure consistently suggest that analysts pick up the styles of their mentors with whom they work in their first jobs as junior analysts. In other

[12] An often-cited incidence is analysts' role in the most recent dot-com bubble in the late 1990s. During the Internet bubble, analysts were under constant pressure to issue favorable stock recommendations for affiliated stocks. After the SEC's prolonged investigation, a Global Research Agreement was finally reached in 2003 between the SEC and ten of the largest investment firms in which the latter paid civil and other penalties of $1.4 billion for their misconduct during the technology-bubble era (http://www.sec.gov/spotlight/globalsettlement.htm).

words, analysts are more optimistic in their future professional careers if they work with optimistic mentors in their early careers. While these rudimentary snapshots support the mentorship experience hypothesis, I further examine in this chapter whether this systematic relationship persists after controlling for optimistic analysts self-selecting into optimistic brokerage houses in their first jobs. More importantly, I conduct a series of exercises to ensure that these results are not a rediscovery nor a manifestation of those analyst characteristics or differences in information environments shown in prior studies that should explain analyst optimism.

While this chapter relates to a growing body of finance and economics studies that show how past experience influences future decisions,[13] the findings in this chapter also relate to recent social network studies showing that social connection has a strong influence in professional domains. For instance, Hong, Kubik, and Stein (2005) document a word-of-mouth effect in which mutual fund managers flock to buy or sell the same stocks held by other mutual fund managers in their cities. Cohen, Frazzini, and Malloy (2010b) find that the recommendations made by analysts who have educational ties with senior firm officers tend to significantly outperform stocks without school ties, providing support for the belief that the "old boy" network commands a positive premium.[14] As the apprenticeship system in the analyst industry also relies on a

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[13] This research addresses a wide range of decision making, including: corporate financial policies (Graham and Narasimhan (2004), Schoar (2008), Graham, Harvey, and Puri (2010)), 401(k) savings (Choi, Laibson, Madrian, Metrick (2009)), inflation expectation (Malmendier and Nagel (2011b)), stock market or IPO entry decisions (Chiang, Hirshleifer, Qian, and Sherman (2011), Kaustia and Knüpfer (2011)), investment and savings decisions (Puri and Robinson (2007), Osili and Paulson (2008)), aggregate consumption (Alesina and Fuchs-Schündeln (2007)), trust (Guiso, Sapienza, and Zingales (2008)), and the investment strategies of mutual fund managers (Greenwood and Nagel (2009)).

[14] Other related papers showing that social ties have an influence on individuals' decision making are as follows: Duflo and Saez (2002), Feng and Seasholes (2004), Hong, Kubik, and Stein (2004), Massa and Simonov (2005), Hong, Kubik, and Stein (2005), Hochberg, Ljungqvist, and Lu (2007), Ivković and Weisbenner (2007), Brown, Ivković, Smith, and Weisbenner (2008), Horton and Serafeim (2009), Hwang and Kim (2009), Cohen, Frazzini, and Malloy (2010a), Cohen, Frazzini, and Malloy (2010b), Engelberg,

connected neighbor structure (between mentors and protégés) for information sharing and dissemination, it is an ideal testground to examine the influence of past experience in this specific nature of social connection.[15]

In fact, establishing the analysts' social networks is critical to answering the research question. The key empirical innovation of this chapter is as follows. Rather than using a social network database, I exploit a unique feature of the widely available IBES (International Brokers Estimate System) dataset: each earnings forecast or recommendation issued by analysts is accompanied by a unique brokerage identifier. Using this brokerage identifying information, I reconstruct the employment timeline of all sell-side analysts in the raw IBES data and trace when and where analysts previously worked. This micro-level panel permits me to address the question of whether analysts are influenced by their mentors. I define *Mentors* as those colleagues who work in the same brokerage house (i.e., same house), cover the same sector (i.e., same team), and have more years of professional experience (i.e., veteran) during analysts' early careers as junior analysts.[16]

The major findings are summarized as follows. First, analysts are more optimistic in their future professional careers if they work with optimistic mentors in their early careers: analysts issue more strong buy recommendations and upgrade jumps, and are more optimistic in earnings forecasts and price targets. Depending on optimism metrics and regression specifications, a one standard deviation increase in standardized mentor

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Gao, and Parsons (2010), Fracassi and Tate (2010), Shive (2010), Gray and Kern (2011), Cohen and Malloy (2011), Kaustia and Knüpfer (2011), Lerner and Malmendier (2011), etc.

[15] This study also relates to Antoniou (2010). However, we address completely different research questions with different identification strategies.

[16] This definition is also supported in anecdotal evidence. For instance, "[a]ccess to a capable colleague who covers a closely related sector matters most. When Lehman Brothers was rated the best research department on Wall Street in the 1990s, its analysts benefited from team-based research processes that heightened their awareness of developments in related sectors and their ability to evaluate such developments knowledgeably." (Groysberg (2010, p.57)).

optimism leads to an increase in standardized analyst optimism by the following magnitudes: 4.341% in *Strong Buys*, 5.935% in *Upgrade Jumps*, 3.955% in *Earnings Forecast Optimism*, and 7.982% in *Price Growth Optimism*. In economic terms, they respectively represent increases in corresponding metrics by 4.70%, 10.04%, 2.17%, and 4.342%, which are economically meaningful and non-trivial.

Specifically, having optimistic mentors in an analyst's early career increases his or her probability of becoming optimistic by 4.303%-8.097%, depending on the regression specifications. In contrast, pessimistic mentors are more likely to influence their protégés to be pessimistic by 1.747% to 5.130%. Optimistic mentors also have an asymmetrically stronger impact than pessimistic mentors. An interesting explanation for these asymmetric impacts would be as follows: if junior analysts who self-select themselves into the equity analyst profession are inherently more optimistic (than the average person) in general, they could be more likely to be influenced by optimistic (versus pessimistic) colleagues in their early careers.

While there is a spillover in optimism, there is no spillover in accuracy. In other words, while it is easy for analysts to pick up their mentors' styles, it is apparently harder for them to pick up their mentors' skills. Only talented superstar mentors (i.e., all-star analysts) can break this pattern, spilling their forecasting skills to their protégés. What is more intriguing is that, controlling for accuracy, optimism, brokerage size, analyst characteristics (i.e., general experience, firm-specific experience), or portfolio complexity (i.e., number of companies, number of industries), analysts with all star mentors in their early careers are almost 6% more likely to be elected as all-star analysts than analysts with no star mentors. This finding suggests that having superstar mentors in analysts' early careers makes a substantial economic difference.

The interesting contrast between style and skills spillover also leads to a more fundamental question about the mentorship system as a whole: are talented analysts born or made? On the one hand, optimism and accuracy spillover evidence suggests analysts only pick up the styles but not the skills of their mentors, whereas only a subset of talented mentors are able to unwind this pattern. On the other hand, the evidence on reputation spillover is intriguing given that analysts' accuracy and other analyst-specific characteristics have been controlled for. A possible explanation to reconcile these findings is that talented mentors transfer more than forecasting skills to their protégés, in whom these skills are demanded by sophisticated institutional investors. The results also consistent with general human capital theories that knowledge and skills are acquired through education and on-the-job learning experience (Rees (1979), Rosen (1981)). The evidence suggests that it simply takes more time than the span of a first job for junior analysts to learn skills from their mentors.[17]

The results above have important implications for regulators (by identifying a new source of optimism), for the analyst profession (by addressing talent management and portability), and for investor (by identifying a simple way to debias analyst optimism); however, it remains unclear as to whether or not investors would care about the visible influence of mentors on their protégés. More specifically, it is interesting to further understand how rational market participants would react to the recommendations made by this group of biased agents, as there is a lack of empirical evidence explicitly examining their interaction in a market setting. I show that the market is smart in identifying the protégés of optimistic mentors, given that the short-run market reaction to their forecast revisions is generally weaker by 15-23 basis points within 10-day window.

[17] The common examples include how to approach firm CEOs, CFOs, competitors or suppliers for better information access, how to extract information from firms' earnings conference calls, or how to analyze firm fundamentals for better forecasts, etc.

Motivated by recent studies that show how sophisticated institutional investors take analyst characteristics into account when digesting the information in their revisions[18], I examine which group of investors is the main driver for the discounting. The results suggest that these weaker reactions primarily concentrate on stocks that are highly held by institutional investors, who discount the revisions made by the protégés of optimistic mentors by 33-38 basis points within 10-day window. This suggests that institutional investors are sophisticated about identifying the apprentices of optimistic mentors and correspondingly discount their revisions; they also take into account analyst characteristics when incorporating the information in their revisions.

To further disentangle treatment effect and selection effect,[19] this chapter adopts a set of falsification tests and econometric exercises. First, to control for analysts' potential selection into brokerage houses due to unobserved brokerage firm culture or incentives, all regressions control for brokerage house fixed effects (Wooldridge (2001, Chapter 10)).[20] All results are robust to different types of brokerage house fixed effects. Second, additional test examining the span of analysts' first jobs also shows that the spillover is stronger among those analysts who spend a longer time with their mentors in their first jobs, thereby supporting the conclusion that analysts are influenced by their mentors via social learning. Third, I further exploit the size of brokerage houses where analysts first

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[18] For instance, Malmendier and Shanthikumar (2007), Hugon and Muslu (2010), and Jiang, Kumar, and Law (2011).

[19] A classic example that differentiates treatment and selection effect is made by a journalist, Malcolm Gladwell ("Getting In" in *The New Yorker*, October 10, 2005): "Social scientists distinguish between what are known as treatment effects and selection effects. The Marine Corps, for instance, is largely a treatment-effect institution. It doesn't have an enormous admissions office grading applicants along four separate dimensions of toughness and intelligence. It's confident that the experience of undergoing Marine Corps basic training will turn you into a formidable soldier. A modeling agency, by contrast, is a selection-effect institution. You don't become beautiful by signing up with an agency. You get signed up by an agency because you're beautiful."

[20] The use of fixed effects coupled with a lagged explanatory variable (i.e., mentor optimism in the current context) is also identical to the approach used in Cohen, Frazzini, and Malloy (2011a, 2011b) and Kaustia and Knüpfer (2011) to mitigate the endogeneity concern.

start their jobs. The idea is that, if self-selection is true, analysts who start their jobs in large brokerage houses simply have more potential mentors to pick and choose from to begin with. If this is the case, the estimated coefficients on *Mentor Optimism* should be stronger for larger than smaller brokerage houses. The results suggest that estimates are not necessarily stronger for larger than smaller brokerage houses. Furthermore, the above systematic pattern remains robust even controlling for a wide selection of mentors' performance- or demographic-related characteristics, and between specialized and diversified analysts.

To further establish causation rather than correlation, I also exploit the fact that each analyst holds a dual role—while analysts are protégés to their seniors, they are also mentors to those rookies who later join the same house. As such, I examine an opposite hypothesis that analysts are influenced by their junior colleagues. Specifically, I estimate the optimism of *Juniors*, who are defined as those house colleagues who cover the same sector but have fewer years of experience during analysts' early careers. I then augment this *Junior Optimism* in the baseline regressions. If juniors spill over their optimism upward (i.e., reverse causality), the estimated coefficient on *Junior Optimism* is expected to be significantly positive. Alternatively, if the systematic pattern between *Analyst Optimism* and *Mentor Optimism* is driven by self-selection (i.e., selection effect), we should also observe a strong and positive coefficient on *Junior Optimism*, as optimistic rookie analysts self-select themselves into optimistic houses. However, the results show no evidence of reverse spillover of optimism from protégés to their mentors. The absence of reverse spillover in optimism further suggests the following conclusions: first, there is a clear causality channel by which analysts learn their mentors' styles during their early careers, but not vice versa. Second, the systematic pattern in optimism spillover cannot be explained by analysts' self-selection in their early careers, as indicated by the lack of spillover in *Junior Optimism*. Collectively, the results suggest that the above systematic spillover cannot be wholly attributed to self-selection.

The results in this chapter are not a rediscovery of those factors shown in prior studies that should explain analyst optimism: all panel regressions control for year effects, industry effects, analyst characteristics and differences in information environment.[21] The inclusion of time fixed effects absorbs any time-series variation in analyst optimism due to labor market conditions, time-varying aggregate risk aversion or market sentiment. Controlling for industry ensures that results are driven by industry-specific variation in analyst optimism. Including analyst characteristics in all estimations ensures that the results are not manifestations of those analyst characteristics that have previously been shown to influence analyst optimism. Leveling the information environment through controlling for information variables also guards against picking up results driven by differences in information environment.

The results survive an extensive list of robustness checks. The results are similar if analysts with thin portfolio coverage are excluded. Excluding IBES data prior to 1993 does not explain away the results.[22] Using current brokerage house fixed effects (instead of first brokerage house) makes the results stronger. The enactment of Regulation FD does not drive away the optimism spillover. Results remain strong and robust even controlling for analysts' year of entry, which could correlate with initial labor market condition or macroeconomic environment, or for year-sector fixed effects, which could correlate with industry-specific sentiment (e.g., the technology industry in late 1990s).

[21] A firm's information environment includes all publicly available information for the firm. An information environment is competitive when there are many information intermediaries who actively collect and disseminate firm information.
[22] Pre-1993 IBES data could be subject to a general delay as the data may not become publicly available on a real-time basis (O'Brien (1988), Zitzewitz (2001), Bernhardt, Campbello, and Kutsoati (2004)).

An alternative estimation methodology based on Fama-MacBeth (1973) regressions also yields similar results.

Results are not driven by research design. First, there is no survivorship bias in the sample: as mentor optimism is historically measured and carried as a static variable in regressions, analyst observations will remain in the sample even when their mentors leave the sample for different reasons. Second, as mentors are defined as analysts who cover the same sector under the current definition, it may be harder for analysts who begin their careers in small brokerage houses to locate mentors as small brokerage houses, unlike large counterparts, have comparatively limited resources and therefore are less likely to engage in clear sector specialization. Using a "hybrid" mentor panel allowing small brokerage analysts to have mentors regardless of whether their mentors cover the same sector, I show that the results are robust to this research design change. Moreover, to ensure that the results do not reflect any mechanical relationships[23] between analyst optimism and past mentor optimism, I conduct a counterfactual simulation. More specifically, analysts are paired up with random mentors. Using this panel of *Pseudo-Mentors*, I show that there is no optimism spillover once the unique mentor-protégé relationship is broken and analysts are teamed up with pseudo-mentors.

The rest of the chapter is organized as follows. Section 3.2 summarizes the sample data, methodology, and main testable hypotheses. Section 3.3 discusses the main results. Robustness tests are summarized in Section 3.4. Conclusion is in Section 3.5.

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[23] From econometrics standpoint, a relationship is mechanical when a common component is present on both sides of a regression (i.e., the dependent and independent variables). A simple analogy would be regressing IBM's stock return on day *t* on its stock return on day *t*. A mechanical relationship could induce biased estimates and give rise to spurious conclusion. In the current context, this test is primarily motivated as there is a shared tag of "optimism" in *Analyst Optimism* and *Mentor Optimism*, despite the fact that they have completely different constructs.

3.2 Sample, Methodology, and Hypotheses

3.2.1 Reconstructing a Panel on Employment History

The reconstruction procedures are outlined below, whereas additional details are provided in Appendix 1. The source of the sample of earnings forecasts (1983-2010), price targets (1999-2010), and stock recommendations (1993-2010) is the Thomson Reuters' IBES dataset. Each earnings forecast, price target or stock recommendation is tagged with the date and unique identifiers identifying the issuing analyst and the brokerage house. Using this unique identifying information, I reconstruct the employment histories of sell-side analysts from 1983 to 2010. An analyst is employed in a brokerage house in month *t* if the analyst issues an earnings forecast under the brokerage house identifier.[24]

Using this panel, I track all other analysts who work in the same brokerage house during an analyst's early career. Recall that *Mentors* are defined as those colleagues who work in the same brokerage house (i.e., same house), cover the same sector (i.e., same team), and have more years of professional experience (i.e., veteran) during an analyst's early career. To mirror the high job mobility of Wall Street analysts in real life, these identified mentors are not required to stay at a given firm throughout the analyst's early career. In other words, these identified colleagues can either enter earlier or leave later than the analyst does. As it is impossible to have complete access to analysts' itineraries on which colleagues they have met, these criteria are designed to maximize the possibilities that analysts do actually interact socially with their mentors during their

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[24] A prior study by Ljungqvist, Malloy, and Marston (2009) documents that historical IBES stock recommendations were altered in the IBES vintages from 2000 to 2007. Thomson Reuters is aware of this problem and, according to the FAQs in WRDS (Wharton Research Data Services), has made corrections for the current recommendation dataset in WRDS. As the stock recommendations used in this study are obtained from the 2011 vintage, the data should be free from this problem. In contrast, according to WRDS, there is no known issue for earnings forecast or price target datasets in prior vintages.

early careers. As previously mentioned, *Early Career* is defined as the analyst's first job or the first three years of the analyst's first job as junior analyst, whichever is shorter, provided that the analyst remains at the brokerage house for at least six months.[25] The choice of a three-year window is arbitrary but identical to that in Hong and Kubik (2003), and it is motivated by the high job-turnover rate observed in the first three years of an analyst's career. Moreover, following Hong and Kubik (2003), an analyst must have at least three years of experience prior to being included in the sample.[26] Analysts are also required to cover at least three firms in any given year.[27] These requirements are imposed to ensure that the results are not driven by extreme values in analyst optimism metrics, as is commonly observed across young analysts who have thin portfolio coverage.[28]

3.2.2 Measuring Optimism

Instead of relying on a single optimism metric, I structure my analyses around four distinct metrics designed to capture different styles of analyst optimism. Using four metrics helps crudely examine if results are sensitive to the particular metric chosen. All four optimism metrics are designed to capture the optimism of an analyst relative to his or her peers.

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[25] The results are not sensitive to these requirements. Results without the six-month minimum requirement are similar. Results without the three-year requirement are slightly weaker. This is expected as the number of panel observation is in negative relationship with this upper threshold (i.e., higher upper threshold and fewer observations lead to lower power).

[26] Anecdotal Wall Street evidence also suggests that junior analysts on average spend three years to get promoted to senior analysts. For instance, Rolfe and Troob (2001, p.9) describe the three-year benchmark: "Following their two- to three-year stint, the vast majority of the analysts will either strike out for any of a handful of graduate business schools, depart the firm for other opportunities within Wall Street's financial community, or regain their sanity and elect to pursue other interests entirely."

[27] As (1) analyst must have at least three years of experience and (2) *Mentors* must have more years of experience than their protégés do, these two requirements collectively make *Mentors* to have at least four years of experience prior to being included in the final sample.

[28] Removing the restriction on analysts to have at least three firms in their portfolios in a given year will make the baseline results stronger.

The first optimism metric is *Strong Buys*, which is computed as the number of strong buy recommendations initiated, upgraded, or reinitiated divided by the total number of outstanding recommendations made by an analyst in a given year. The weight ranges from 0.0 (i.e., no strong buy recommendations) to 1.0 (i.e., only strong buy recommendations). Optimistic analysts are expected to issue more strong buy recommendations, controlling for other known analyst characteristics or variables on information environments.

The second metric is *Upgrade Jumps*, which is computed based on the number of upgrade jumps to the total number of recommendation revisions made by an analyst in a given year. A revision is regarded as a jump if the revision in recommendation rating for a given firm is not to its immediately adjacent rating category. For instance, an upgrade is a jump when the rating is upgraded from "Sell" to "Buy."[29] Optimistic analysts are expected to issue more upgrade jumps. Like *Strong Buys*, this metric ranges from 0.0 (least optimistic) to 1.0 (most optimistic). As both *Strong Buys* and *Upgrade Jumps* are constructed based on the recommendations issued in a given year, it is effective to put in year fixed effects when constructing these relative measures.[30] Moreover, as *Upgrade Jumps* could lead analysts to issue more strong buys in a given year, these two metrics should mildly correlate in the data.

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[29] Following IBES classification, a recommendation ranges from Strong Buy, Buy, Hold, Sell, to Strong Sell.

[30] While *Upgrade Jumps* denotes changes in analyst opinion, it does not depend on earnings announcements or issuances of management guidance for three main reasons. First, *Upgrade Jumps* conditional on a public signal effectively removes a substantial portion of upgrades that could contain private information. Second, it is not entirely clear to determine *ex ante* the length of the response window. Analysts who are slow to respond outside a prescribed event window may be misclassified. Third, the main objective of the optimism metrics is to comprehensively analyze analysts' styles without depending on information, which could complicate the result interpretation.

The third optimism metric is the percentile-rank optimism, *Earnings Forecast Optimism*.[31] An analyst's *Earnings Forecast Optimism* on stock *j* at time *t* is calculated as follows:

$$Earnings\ forecast\ optimism_{i,j,t} = 100 - \left[\left(\frac{Rank\ of\ forecast\ error_{i,j,t} - 1}{Number\ of\ analysts\ following\ stock\ j - 1}\right)\times 100\right] \qquad (1)$$

Rank of forecast error is a rank variable based on the rank of an analyst's forecast error depending on one-year-ahead EPS forecasts. The most (least) optimistic analyst covering stock *j* in time *t* would be given the first (last) rank. As this metric is conditional on the same firm-year, this is identical to controlling for firm-year fixed effects. Following Hong and Kubik (2003), this measure is calculated on a three-year rolling window. To reduce extreme values made by analysts who maintain thin portfolio coverage, I further rank this metric and assign a value between 1 (least optimistic) and 100 (most optimistic).[32] It is also important to control for forecast horizon when using this measure as analysts tend to walk down their earnings forecasts during the fiscal year for management to beat (Richardson, Teoh, and Wysocki (2004)).

The last metric is *Price Growth Optimism*, which is computed based on the one-year-ahead split-adjusted price target to the split-adjusted stock price on announcement date. Similar to *Earnings Forecast Optimism*, I rank this metric each year and assign a

[31] This metric is commonly used in the analyst literature (e.g., Hong, Kubik, and Solomon (2000), and Hong and Kubik (2003)). The main advantage of using percentile-rank optimism is that the optimism of analysts is compared to that of their peers. This stands in contrast to *Absolute Optimism*, which is defined as the difference of earnings forecasts above actual earnings. Under *Absolute Optimism*, analysts covering stock *j* can all be optimistic as long as their forecasts are above actual earnings. As such, percentile-rank relative optimism compensates this shortcoming and analysts are ranked relative to their peers in which by construct a portion of analysts must be optimistic/pessimistic. Results based on absolute optimism are reported in Section 5.4.

[32] My results are not sensitive to this requirement as the baseline results would be similar or sometimes stronger without this treatment.

value between 1 (least optimistic) and 100 (most optimistic), thereby creating a bound for this metric to ensure that it is not driven by extreme values.

3.2.3 Mentor Optimism

Metrics of mentor optimism are constructed in the same manner. The only difference is that all mentor optimism metrics are snapshots measured during analysts' first jobs. Specifically, for each analyst, his or her mentor optimism is computed using the average of all mentor-year optimism metrics. This resulting measure is then carried forward as a static explanatory variable.

Figure 3.2 illustrates an example of the timeline on *Mentor Optimism*. For instance, if an analyst spends his or her first three years (from years t to t+2) in a brokerage house, *Mentor Optimism* is measured based on the optimism of the mentors in the analyst's first brokerage house during years t to t+2. To ensure that *Analyst Optimism* and *Mentor Optimism* are not endogenous, *Analyst Optimism* is only measured the year after his or her first job from years t+3 and onward. Specifically, *Analyst Optimism* is only measured at least a year after his or her first job. In short, those optimism metrics are not measured simultaneously but sequentially, where historical *Mentor Optimism* is measured and carried forward as a fixed, static explanatory variable in all regressions.

3.2.4 Main Testable Hypotheses

Motivated by the literature summarized in the preceding sections, I conjecture that the past mentorship experience of Wall Street analysts has an impact on their professional careers and posit the following testable hypotheses:

H1: Analyst optimism is influenced by the optimism of their mentors in their early careers.

H1a: Analysts are more optimistic if they work with optimistic mentors in their early careers.

H1b: Analysts are more likely to be optimistic if they work with optimistic mentors in their early careers.

While *H1a* and *H1b* ask similar questions, there is a subtle difference between them: *H1a* uses panel regressions to examine whether there is a systematic relationship between past mentor optimism and future analyst optimism, whereas *H1b* uses logistic regressions to investigate whether analysts would be more optimistic if they start their careers with optimistic mentors.

While the first hypothesis projects whether analysts' professional styles are influenced by their mentors, the immediate question is whether investors would care about the visible influence of mentors on their protégés. Specifically, it is unclear whether the market would benefit from identifying the protégés of optimistic mentors. As such, I further conjecture that the market is smart in identifying optimistic analysts based on their past mentors:

H2: The market is smart in identifying the protégés of optimistic mentors.

H2a: Short-run market reactions to forecast revisions made by analysts who work with optimistic mentors in their early careers are weaker.

3.3 Main Results

In this section, I first provide a brief descriptive statistics summary. Next, I present the main results, showing that analyst optimism is influenced by their mentors in their early careers. Further evidence on the influence of optimistic mentors, skills spillover, market reaction, and causality of optimism spillover is then summarized.

3.3.1 Brief Statistics Summary

Panel A of Table 3.1 reports the summary statistics of the main variables in this study. In a given year, 24% of analysts' recommendations are strong buys and 13% of their recommendation upgrades are upgrade jumps. Analysts also tend to have similar levels of optimism in earnings forecasts and price targets to those of their mentors.

Panel B of Table 3.1 summarizes the correlations among different optimism metrics. Conditional on the same analysts, analysts who hold more strong buy recommendations tend to issue more large recommendation upgrades in a given year, as *Strong Buys* and *Upgrade Jumps* are both mildly correlated (correlation=0.557).[33] In contrast, *Price Growth Optimism* exhibits little correlation with *Earnings Forecast Optimism* (correlation=0.014), and the correlation is not significant at the 10% level. In general, mentor optimism metrics generally show modest correlations with protégés' optimism metrics (i.e., 0.060 of *Earnings Forecast Optimism* to 0.220 of *Price Growth Optimism*).[34]

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[33] This supports an earlier conjecture that upgrade jumps could lead analysts to issue more strong buys (Section 2.2) as these two optimism metrics are mildly correlated with each other.

[34] Hong and Kubik (2003) show that from 1983 to 2000 an analyst's relative optimism and accuracy are negatively correlated (about -0.18), which I am able to replicate (correlation: -0.162); however, an analyst's relative optimism and accuracy are not necessarily negatively correlated by design. A simple example would illustrate this point. Assume two analysts (A and B) make forecast errors of +0.5 and -0.5, respectively. While they both score 100 in percentile-rank accuracy, analyst A (B) would score 100 (0) in percentile-rank optimism. In other words, an analyst's relative optimism and accuracy are not necessarily a hard-wired relationship. Raw data also suggest a similar conjecture, as the correlation of optimism and accuracy is positive (correlation: 0.0783) from 2001 to 2010.

Panel C of Table 3.1 summarizes the number of analysts and their mentors in the sample.[35] Over the 26 years from 1985 to 2010, the final sample covers about 40% of analysts in IBES.[36] For analysts who start their careers in a given year, they generally work with 3.73 mentors, who are veteran analysts with more years of professional experience and cover the same sector in the same firm. As Wall Street brokerage houses are sharpening their business focus by specializing in certain sectors (Clement and Tse (2005), Sonney (2009)), the average number of mentors (6.33) is slightly higher than the median (3.73).

3.3.2 Baseline Specifications

To examine whether analyst optimism is influenced by mentor optimism, I run the following baseline panel regression:

$$Analyst\ Optimism_{i,t} = \alpha + \beta_1 Mentor\ Optimism_i + \beta_2 X + \beta_3 Fixed\ Effects + e_{i,t} \tag{2}$$

For each analyst i in year t, I regress the analyst i's optimism on *Mentor Optimism*, a k-vector a ($K\times1$) of control variables (X), and a set of fixed effects controlling for differences in information environments. *Mentor Optimism* does not have a subscript of time t since it is a snapshot taken during analysts' first jobs as junior analysts. The main coefficient of interest is β_1 as it captures the influence of past mentor optimism on analyst optimism. X is a set of control variables including a host of known analyst characteristics or other variables that have been shown in prior literature to have

[35] While the IBES dataset starts in 1982, the sample period starts years later in 1985, as analysts are required to have at least three years of experience prior to being included in the sample (Hong and Kubik (2003)).

[36] Unreported tests show that the raw data on the number of IBES analysts are similar to those reported in Cohen, Frazzini, and Malloy (2010b).

an influence on analyst optimism.[37] They include analyst forecast accuracy (*Accuracy*), number of companies, number of industries, general experience, firm-specific experience, all-star analyst status, the proportion of affiliated clients (*Underwriting*), days to year-end,[38] and firm coverage (i.e., the average analyst coverage of an analyst's portfolio of firms).[39] Fixed effects refer to time and industry fixed effects.[40] Putting in these fixed effects serves to absorb any time-variation in the optimism metrics over sample years. This is done to ensure that the results do not capture business cycles, enactment of regulations (e.g., Regulation FD) or industry-specific environment (e.g., Griffin, Harris, Shu, and Topaloglu (2011)).[41] To address the concern that optimism metrics are likely to be correlated within analysts, all standard errors are clustered at analyst level.[42]

As *Mentor Optimism* is measured during analysts' early careers, it is possible that optimistic analysts in their early careers self-select themselves into optimistic brokerage houses. If that is the case, an endogeneity issue could arise as metrics on mentor optimism could be endogenous (McNichols and O'Brien (1997)). To mitigate the

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[37] For instance, O'Brien (1988), Klein (1990), Clement (1999), Richardson, Hong, and Wysocki (2004), Clement and Tse (2005), and Clement, Koonce, and Lopez (2007).

[38] Clement (1999) shows that absolute earnings forecast error increases when day to fiscal year-end increases. Studies by O'Brien (1988) and Klein (1990) also show that analyst optimism declines throughout the firms' fiscal year.

[39] Firm coverage assesses how crowded analyst coverage is for a stock.

[40] Specifically, industry fixed effects are constructed based on IBES sectors (13 of them).

[41] While it would be ideal to put in mentor fixed effects, it is not possible to do so for two reasons. First, there are multiple mentors for each analyst. As such, it is not entirely clear whether to put in one fixed effect for each mentor or for each group of mentors. Second, putting in high-dimensional mentor fixed effects would quickly run into the "curse of dimensionality" problem, as the number of mentor fixed effects may approach cross-sectional observations in early years, thereby quickly exhausting the degree of freedom. Besides, instead of controlling for first brokerage house fixed effects, the above regression could control for research director fixed effects as research directors may have a direct influence on analysts' outputs. However, collation of such data proves to be difficult as data on research directors are not available for years earlier than 1996, and they require substantial manual matching.

[42] The results reported in Tables 3.2 and 3.3 are not sensitive to the non-linearity in control variables. Specifically, I transform control variables including *Number of companies*, *Number of industries*, *Days to fiscal year-end*, *Firm coverage*, *Brokerage size*, *Firm-specific experience* into non-linear variables by taking a natural logarithm of one plus the variable of interest. The results based on these non-linear control variables are almost identical to those reported in Tables 3.2 and 3.3.

endogeneity concern in mentor optimism, which is a snapshot taken when they are in their first brokerage houses, I have included first brokerage house fixed effects in all panel regressions, which take a value of one for each house that analysts and mentors have both belonged to during their early careers (Wooldridge (2001, Chapter 10)).[43] Moreover, it is important to note that the sample begins after analysts' first jobs to avoid any contemporaneous estimation.

3.3.3 Main Results: Optimism Spillover

The baseline results on optimism spillover are reported in Tables 3.2 and 3.3. In Table 3.2, *Strong Buys* and *Upgrade Jumps*, two analyst optimism metrics, are regressed on their corresponding mentor optimism metrics. The results show that across all specifications, there is a strong systematic relationship between analyst and past mentor optimism while controlling for other factors. For *Strong Buys* under Columns (1)-(4), the coefficients are significantly positive, ranging from 4.341% to 6.276% (*t*-statistics from 2.36 to 3.47). Significantly positive results are also observed for *Upgrade Jumps* under Columns (5)-(8): the estimated coefficients range from 5.935 to 6.983 (*t*-statistics from 3.12 to 3.54). Since all independent and dependent variables have been standardized with mean zero and unit standard deviation, it is easy to translate these coefficients into economic magnitudes: a one standard deviation increase in *Strong Buys* leads to a 4.70% increase in *Strong Buys*.[44] On the other hand, a one standard deviation increase in mentor

[43] As previously mentioned, the use of fixed effects coupled with a lagged explanatory variable is identical to the approach used in Cohen, Frazzini, and Malloy (2011a, 2011b) and Kaustia and Knüpfer (2011) to mitigate the endogeneity concern.

[44] Specifically, a one standard deviation increase in *Strong Buys* leads to a 1.129% (=4.341%×0.26) increase in analyst *Strong Buys*. Relative to the mean of analyst *Strong Buys* of 0.24, that would translate to about a 4.70% increase in *Strong Buys*.

Upgrade Jumps leads to a 10.04% increase in *Upgrade Jumps*, which is economically significant.

A similar systematic spillover is observed using two other metrics based on earnings forecasts (Columns 1-4) and price targets (Columns 5-8) in Table 3.3. All specifications in Table 3.3 regressing *Earnings Forecast Optimism* and *Price Growth Optimism* on their historical mentor counterparts produce statistically strong and positive coefficients. The estimated coefficients on mentor *Earnings Forecast Optimism* range from 3.955% to 4.175%. As such, a one standard deviation increase in mentor *Earnings Forecast Optimism* leads to an increase ranging from 2.17% to 2.24%. In addition, the estimated effects of mentor *Price Growth Optimism* range from 7.982% to 11.890%. The economic significance of those estimates ranges from 4.342% to 6.466%, which again are economically significant findings.

Using standardized variables permits us to directly address the question of which explanatory variable has a larger explanatory power. Mentor optimism plays a non-trivial role in explaining future analyst optimism: the absolute magnitudes of the metrics on mentor optimism are ranked as the second most important determinants in *Strong Buys*, *Upgrade Jumps*, and *Price Growth Optimism*, and the fourth most important in *Forecast Earnings Optimism*. For other determinants, brokerage size unsurprisingly has a strong deterring effect on analyst optimism. This is because brokerage size is positively related to the firm's reputation, and large brokerage houses would be expected without the need for their analysts to exhibit excessive optimism. This finding is consistent with the evidence in Das, Levine, and Sivaramakrishnan (1998), Lim (2001), and Cowen, Groysberg, and Healy (2006). On the other hand, consistent with the finding in Clement (1999), *Days to Year-end*, which describes the timeliness of earnings forecasts, has a strong and positive influence on analyst optimism.

Together these results provide evidence suggesting that mentor optimism explains a substantial fraction of variation in analyst optimism. The findings offer strong support that analyst optimism is influenced by the optimism of their past mentors in their early careers (*H1*) and analysts are more optimistic if they work with optimistic mentors in their early careers (*H1a*).

3.3.4 Influence of Optimistic and Pessimistic Mentors

To examine the influence of optimistic and pessimistic mentors, I run the following logistic regressions:

$$Logit[\Pr(Optimistic\ analyst_{i,t} \mid X, Optimistic\ mentors_i)] = \alpha + \beta_1 Optimistic\ mentors_i + \beta_2 X + \beta_3 Fixed\ \ effects + e_{i,t} \quad (3)$$

I define *Optimistic Analysts* as a dummy that takes one if an analyst is in the top quintile of a given optimism metric. *Optimistic Mentors* are defined in a similar fashion, taking one when mentor optimism is either in the top 10% or 20% in a given year. The major difference between Equations (2) and (3) is that all explanatory variables that enter into the logistic regressions are dummies to control for the potential impact resulting from the non-linearity in explanatory variables.[45]

Panel A of Table 3.4 reports the impact of having optimistic mentors. Having mentors in the top 10% of optimistic mentors in an analyst's early career increases his or her probability of becoming optimistic by the following marginal probabilities, all of which are statistically significant at reasonable levels: 8.097% on *Strong Buys*, 6.579%

[45] This is similar to the approach used by Hong and Kubik (2003). Specifically, Equation (3) can be regarded as an extended version of Hong and Kubik's approach, as I have included extra variables as control variables. In short, there are about 170 fixed effects in these logistic regressions (see the even-numbered Columns in Table IV).

on *Upgrade Jumps*, 4.303% on *Earnings Forecast Optimism*, and 5.283% on *Price Growth Optimism*, respectively. Results (reported in the last two rows in Panel A) are weaker but similar if I apply an alternative definition of optimistic mentors: expanding optimistic mentors to include the top quintile (instead of top decile) only marginally weakens the results.[46]

In addition to looking into optimistic mentors, I examine whether I would observe the direct opposite effect of pessimistic mentors. I interchange optimistic dependent dummies (mentors) with pessimistic dependent dummies (mentors) and re-run the baseline specifications. Logistic regression results reported in Panel B provide support that analysts are more likely to be pessimistic if they work with pessimistic mentors in their early careers. Except for *Strong Buys*, the estimated coefficients are all positive and statistically significant for six out of the eight regressions. Using an alternative definition of pessimistic mentors (including the top quintile instead of the top decile) makes the results, including the results on *Strong Buys*, even stronger. On average, analysts are 2.982%-8.918% more likely to become pessimistic when they work with pessimistic analysts in their early careers. Panel C includes both optimistic and pessimistic mentors in logistic regressions, showing that as expected analysts are more likely to be influenced to be optimistic by early optimistic than pessimistic mentors. Unreported results are similar but slightly weaker if I use the alternative definitions based on top quintile.

In addition to the findings above, results in Table 3.4 suggest that optimistic mentors have an asymmetrically stronger impact than pessimistic mentors. This is

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[46] In unreported results on Earnings Forecast Optimism, I find that protégés are more likely to become optimistic if their mentors are strong in earnings accuracy. Interacting the top 10% indicators on *Mentor Earnings Forecast Accuracy* and *Mentor Earnings Forecast Optimism*, the estimated marginal probability on the interaction term is 6.341%. In other words, strong mentors, who are both historically more accurate and optimistic, are more likely to influence analysts to become optimistic analysts than their weak counterparts.

suggested by the observation that the average marginal probabilities of optimistic mentors (7.20%-9.03% in Panel A) are larger than those of pessimistic mentors (4.98%-6.18% in Panel B). One potential explanation is as follows: if analysts who self-select themselves into the equity analyst profession are more optimistic (than the average person) in general, they are more likely to be influenced by optimistic (rather than pessimistic) colleagues in their early careers.[47]

In short, the above results suggest that the styles of early mentors have a strong influence on analysts' styles. Analysts are more likely to be optimistic (pessimistic) if they work with optimistic (pessimistic) mentors in their early careers (*H1b*).

3.3.5 Skills Spillover

While the above sections provide evidence showing that analysts carry over their early career experience into their professional careers, this evidence focuses on whether analysts pick up the *styles* of their mentors. The next immediate question is whether analysts pick up the *skills* of their mentors. While skills in other professions may be harder to define, skills in the current context can be easily measured and compared by looking into the accuracy of their earnings forecasts. As such, I examine the spillover in forecast accuracy in this section to see whether analysts inherit the skills of their mentors.

Table 3.5 reports the results of regressing analyst accuracy on mentor accuracy. The regression specifications are identical to those in Table 3.2 except the main dependent (*Analyst Optimism*) and independent variables (*Mentor Optimism*) are replaced with *Analyst Accuracy* and *Mentor Accuracy*, respectively. The regression results in

[47] The idea is consistent with Seligman (1998), who shows that optimistic people are less influenced by negative events, as optimistic people tend to attribute these negative externalities to temporal, uncontrollable factors.

Table 3.5 suggest that there is little or no spillover in forecast accuracy as all of the estimated coefficients are statistically insignificant.

Whereas the results suggest that there is no spillover in accuracy, there are two implications once we line up the results between optimism and accuracy spillover. First, while it is easy for analysts to pick up mentors' styles, it is apparently harder for them to learn the skills of their mentors. It simply takes more time than the span of a first job for junior analysts to pick up the skills from their mentors. Second, these results are consistent with the main idea behind the study of Bikhchandani, Hirshleifer, and Welch (1992) in which "individuals rapidly converge on one action on the basis of some but very little information" (p.994). In the current context, young analysts pick up the styles of their mentors even though they do not have complete information about their mentors' actions.

Here is an alternative interpretation of the above results: similar to the debate in the literature on active mutual fund managers,[48] if sell-side analysts *on average* do not have superior abilities to forecast earnings, there should be no spillover in accuracy for average mentors to their mentees in general. As such, it is possible that only a handful of talented mentors can transfer their professional skills to their protégés. I examine this conjecture and conduct the following analysis. As prior literature shows that all-star analysts on average are more accurate in making earnings forecasts, I locate those all-star analysts by looking them up in the *Institutional Investor* analyst rankings. After constructing an *All-Star Mentor* dummy indicating whether at least one of the analyst's mentors has ever been ranked as an all-star analyst in the *Institutional Investor* magazine since 1983, I interact mentor accuracy with this *All-Star Mentor* dummy. If analysts are

[48] Grinblatt and Titman (1992), Carhart (1997), and Kacperczyk, Sialm, and Zheng (2005).

able to pick up skills from superior mentors, this interaction variable should be positive, reflecting that skilled mentors are more capable of passing their knowledge and skills to their protégés.

Panel B reports these interaction results. The evidence suggests that all-star mentors are better at passing their superior skills to their protégés. The estimated coefficients of *All-Star Mentor* × *Mentor Accuracy* are at least more marginally statistically positive. A one standard deviation increase in *All-Star Mentor* × *Mentor Accuracy* leads to 1.575% to 4.014% increase in analyst accuracy, depending on the regression specifications.[49] That would also translate into a 19.6% to 28.5% increase in analyst accuracy relative to the explanatory power (i.e., economic magnitude) of *All-Star analyst* indicator, which is economically meaningful and non-trivial. To better put these results into context, one should consider two stylized facts of all-star analysts. First, all-star analysts are more accurate in earnings forecast than non-star analysts. Regressing analysts' percentile-rank accuracy on all-star status gives a significantly positive coefficient (7.014% in standardized accuracy; *t*-stat: 19.10).[50] Second, the superiority in forecast accuracy for all-star analysts persists, albeit weaker, even after the years when they are no longer voted as all-star analysts. Regressing an analyst's percentile-rank accuracy on (a) all-star status and (b) an *Ever Star* dummy, which takes one whenever an analyst has ever been voted as all-star analysts, gives a significantly positive coefficient on *Ever Star* (5.523%; *t*-stat: 10.82). These stylized facts suggest that all-star analysts are in general more accurate than non-star analysts. Together with these two stylized facts,

[49] The estimated coefficients of *General experience* and *Firm-specific experience* in both panels are all positive and statistically significant, suggesting that the analyst's accuracy tends to increase if his or her professional or firm-specific experience increases. This is consistent with the findings of McNichols and O'Brien (1997), Mikhail, Walther, and Willis (1999), Clement (1999), Jacob, Lys, and Neale (1999), and Clement, Koonce, and Lopez (2007).

[50] This is also consistent with Stickel (1992), Leone and Wu (2007), and Fang and Yasuda (2010).

the results on interaction suggest that only talented all-star mentors, who are more accurate in earnings forecasts, are able to pass on their superior accuracy skills to their protégés.

While prior studies suggest that accurate analysts are more likely to attain all-star analyst status (e.g., Stickel (1992), Leone and Wu (2007), and Fang and Yasuda (2010)), it is not clear whether having superior mentors would help their protégés attain the all-star analyst status. I find that having mentors in analysts' early careers make a substantial difference in their future professional development. Panel C in Table 3.5 reports the marginal probabilities from regressing all-star analyst dummy on a host of variables. The main variable of interest is *Proportion of All-Star Mentors*, which is defined as the proportion of all-star mentors in an analyst's early career. These results suggest that having all-star mentors in one's early career makes a big difference for the protégé's subsequent career: analysts who have all-star mentors in their early careers are 5.933% more likely to be elected as all-star analysts than analysts with no star mentors, controlling for their accuracy, optimism, brokerage size, and other known analyst characteristics. If an analyst were to move from the 25th to the 75th *Proportion of All-Star Mentors* while other analyst characteristics remain constant, the analyst's probability of being ranked as an all-star analyst increases by 1.48%. Sub-period results are fairly consistent as shown in Columns (3) and (4). Given that all specifications control for analyst accuracy, the incremental probabilities shown above are surprising as they indicate that reputation spills over from mentors to protégés. Thus, having all-star mentors in their early careers significantly impacts the protégés' future professional success.[51]

[51] The estimated marginal probabilities on analyst accuracy in Panel C are all positive and statistically significant, suggesting that accurate analysts are more likely to be voted as all-star analysts. The results are consistent with those in Stickel (1992), Leone and Wu (2007), and Fang and Yasuda (2010).

Together these results provide additional insights into why numerous Wall Street brokerage houses spend enormous resources trying to recruit top-notch, all-star analysts from rival brokerage houses—attracting big stars (i) enhances the research quality of and (ii) spills over their reputation to those analysts in the hiring brokerage houses. The interesting contrast between style and skills spillover also leads to a more fundamental question on the mentorship system as a whole—are talented analysts born or made? On the one hand, optimism and accuracy spillover evidence suggests analysts only pick up the styles but not skills of their mentors, whereas only a subset of talented mentors are able to unwind this pattern to pass their forecast skills to their protégés. On the other hand, the evidence on reputation spillover is intriguing since analysts' accuracy and other analyst-specific characteristics have been properly controlled for. A possible explanation for reputation spillover is that talented mentors transfer to their protégés skills that do not relate to forecast accuracy but are demanded by sophisticated institutional investors.

3.3.6 Robustness Checks and Other Tests

The following section summarizes the results of a battery of robustness checks, followed by the results of counterfactual simulation and additional analyses.

3.3.6.1 Robustness Checks

Table 3.6 reports the results of different robustness checks. "*Active analysts only*" focuses on active analysts who cover at least eight firms per year. The idea of active analysts is to see if the spillover in optimism merely results from analysts who have thin coverage in their research portfolios. The results are similar to the baseline results.

"*After 1993*" excludes forecasts made before year 1993 as prior studies suggest that pre-1993 IBES data may not become publicly available on a real-time basis and could be subject to a general delay (O'Brien (1988), Zitzewitz (2001)).[52] Note that recommendation data are only available after 1993; as such, the "*After 1993*" test mainly focuses on the earnings forecast and price target data. The results reported in Table 3.6 again are similar to the baseline results and are not driven by this potential timing problem.

While there is a remote possibility that these style-based results on optimism spillover are driven by information disclosure, "*After Regulation FD*" examines this possibility and investigates whether the results are driven by the advanced disclosure to selective participants prior to Regulation Fair Disclosure (Reg FD) in 2000.[53] I show that the results based on the post-Reg FD sample are again similar to those baseline estimates.

"*Current brokerage house fixed effect*" reports results controlling for current brokerage house fixed effects instead of first brokerage houses. The idea is to test whether optimistic analysts self-select themselves into optimistic brokerage houses. The results are again similar to those baseline estimates, thereby suggesting that these results on optimism spillover cannot be explained by self-selection of optimistic analysts into optimistic brokerage houses.[54]

"Y*ear of entry fixed effect*" examines whether the optimism spillover reflects the initial labor market condition when analysts first enter into the sell-side analyst industry. A study by Oyer (2008) suggests that the initial job market condition, as proxied through

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[52] O'Brien (1988), Zitzewitz (2001), and Bernhardt, Campbello, and Kutsoati (2004).

[53] Gintschel and Markov (2004), Ke, Petroni, and Yu (2008), Bernile, Kumar, and Sulaeman (2011).

[54] While the alternative would be to include house optimism based on the contemporaneous optimism of the colleagues in current brokerage houses (i.e., house optimism), I do not pursue such a strategy since analyst and house optimism are clearly endogenous. As such, other estimated coefficients would be biased during estimation.

the stock market condition, could have a large impact on new MBA students seeking Wall Street employment; thus, I control for the year of entry to examine this possible hypothesis. Moreover, controlling for year of entry reveals whether the results are driven by the initial market sentiment when analysts first join the profession. As such, in the regressions I include "*Year of first entry fixed effect,*" which is a series of dummies each taking a value for the year when analysts first appear in the IBES dataset. Again, the main message of the baseline results remains unchanged after controlling for years of analysts' first entries.

"*Year-sector fixed effects*" includes year-sector fixed effects in regressions (instead of year fixed effects and sector fixed effects in baseline regressions). This check is motivated by anecdotal evidence that industry sentiment could be exceptionally high in certain periods (e.g., the technology bubble in the late 1990s).[55] The results suggest that the baseline results are not driven by this concern.[56]

"*Fama-MacBeth (FM) regressions*" report the regression results estimated using annual time-series cross-sectional Fama-MacBeth (1973) regressions. Except for year and first brokerage house fixed effects which are not included, the regressions for the annual FM regressions are identical to those in Tables 3.2 and 3.3. Standard errors are adjusted using Newey-West (1987) with a 4-year lag. All results remain positive and significant under FM regressions. The economic magnitudes appear to be even stronger than those in the baseline results. Looking into the signs of estimated coefficients further reveals that a majority of the estimated coefficients are both positive and statistically significant.[57]

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[55] For instance, Brunnermeier and Nagel, (2004) and Griffin, Harris, Shu, and Topaloglu (2011).

[56] Except for *Price Growth Optimism*, unreported results show that all results remain robust even controlling for current brokerage house and sector fixed effects.

[57] Looking into analysts who move from the most (least) optimistic to the least (most) optimistic brokerage houses, I also examine whether they still carry their early career experience. However, as it is not common for such career moves in the sample, the thin sample does not offer sufficient observations for testing.

3.3.6.2 Counterfactual Simulation: Pseudo-Mentors Panel

A potential explanation to the above results is that there is an unobserved, mechanical relationship underpinning analyst and mentor optimism. As previously mentioned, from econometrics standpoint a relationship is mechanical when a common component is present on both sides of a regression. While the main dependent (i.e., *Mentor Optimism*) and independent (i.e., *Analyst Optimism*) variables are based on completely different constructs, the impression that they share a common tag of "optimism" may raise concern that the above results are spurious.

To tackle this concern, I exploit the panel on employment history and conduct the following counterfactual simulation: if there is a hard-wired relationship between analyst and mentor optimism, we should still observe strong results even when we assign analysts to random mentors. In contrast, if the strong results documented above arise from the mentor-protégé relationship but are not mechanical in nature, the results should disappear once we mix and match analysts with random mentors. As such, I conduct a randomized experiment using the following steps. First, I match analysts with mentors randomly drawn from the sample without replacement. The final deliverable is a simulated panel on employment history in which analysts are matched with *Pseudo-Mentors*. Second, after assembling this pseudo-mentor panel, regressions are run with the optimism metrics of the pseudo-mentors (i.e., *Pseudo-Mentor Optimism*) with the control variables and fixed effects identical to those in preceding tables. Finally, I repeat this two-step randomization 1,000 times.

Figure 3.3 further plots the distributions of estimated coefficients on *Pseudo-Mentor Optimism*. In Figure 3.3 the distributions of estimated coefficients on *Pseudo-*

Mentor Optimism largely cluster around zero (i.e., no systematic relationship). The average coefficients show that none of the estimated coefficients are significantly loaded (neither positive nor negative).[58] In other words, the economic magnitudes from *Pseudo-Mentor Optimism* on analyst optimism are on average non-existent. Together these findings suggest that the results documented above are not spurious in nature—i.e., not capturing an unobservable mechanical relation in the data—as the results generated under the pseudo-mentor panel are not even remotely similar. Moreover, they suggest that the nature of social ties between analysts and their mentors is unique and important in explaining the results of optimism spillover.

3.3.6.3 Small Brokerage Houses: A Hybrid Estimation

A potential objection to requiring mentors and protégés to cover the same sector is as follows: under the existing definition of mentors, it is harder for analysts who start their careers in small brokerage houses to have *Mentors*. Unlike large brokerage houses, small brokerage houses have comparatively limited resources and are less likely to engage in clear sector specialization. In other words, it is more likely for analysts in small brokerage houses to contemporaneously cover several sectors. The immediate consequence is that analysts in large houses could take a larger weight in the final sample.

To address this potential concern, I re-design the regression specification. Specifically, for those small brokerage houses where analysts do not have mentors who cover the same sectors in their early careers, their missing mentor optimism metrics are

[58] Specifically, the estimated coefficients are summarized as follows with *t*-statistics in parentheses: 0.009% (0.01) for *Strong Buys*, -0.032% (-0.05) for *Upgrade Jumps*, -0.051% (-0.07) for *Earnings Forecast Optimism*, and 0.011% (0.01) for *Price Growth Optimism*.

replaced with the optimism metrics generated based on all other seniors regardless of whether they cover the same sectors. The idea of this “hybrid” mentor optimism is to determine to what extent the results are driven by the condition that both analysts and mentors are required to cover the same sectors.

Panel A of Table 3.7 reports the baseline results estimated under this hybrid estimation for brokerage houses with fewer than five employees. In general, the results are similar to the baseline results if we include more analysts who start their careers in small brokerage houses. As such, the results suggest that optimism spillover is not driven by the research design where both analysts and mentors are required to cover the same sector. I also use alternative definitions of small brokerage house and report the results in Panel B. Again, results are generally similar: except for *Strong Buys*, the significance of other metrics generally decreases when the upper threshold of brokerage size increases. It is also interesting to note that in Panel B, once I include more “irrelevant” senior analysts who do not cover the same sector, the economic magnitudes of the spillover gradually decrease. Similar to the results reported under the counterfactual simulation in Online Appendix 3, this pattern suggests that there is a unique mentor-protégé relationship that plays an important role in explaining the economic channel of optimism spillover.

In short, the results in Table 3.7 suggest that the findings above are not driven by the potential identification concern for analysts who start their careers in small brokerage houses.

3.3.6.4 Location Proximity

As large brokerage houses may maintain branches, one potential concern is that mentors and their protégés could work in different cities. If this were the case, it would be more difficult for protégés to learn from their mentors through daily social interactions.

To address this concern, I first hand-collect brokerage house locations in *Nelson's Directory of Investment Research*. Figure 3.4 is a snapshot of brokerage house location (including all headquarters and branches) at the end of 2008. Over 60% of those 1,913 analysts employed by 193 brokerage houses are situated in four major financial hubs: New York (51.5%), San Francisco (7.4%), Chicago (3.5%), and Boston (3.3%), respectively. Specifically, 65% of brokerage houses have their analysts situated in their houses' headquarters, regardless of whether they maintain branches. The concentration may indicate the advantage of information pooling. Moreover, the estimated coefficient from regressing the number of branches on the number of analysts is 0.074 (*t*-6.37). If House A hires 10 more analysts than rival House B, the former is expected to have 0.7 more branches than the latter.[59]

To further address this concern, I conduct a simple inference exercise. If location proximity between mentors and protégés inversely influences optimism spillover (i.e., long distance leading to weaker optimism spillover), results should be weaker in the

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[59] While it may be possible to hand-match each analyst from *Nelson's* to IBES, there are a few empirical challenges. First, as *Nelson's Directory of Investment Research* only provides detailed location data on or after 1996, a substantial portion of data prior to 1996 would be dropped from the matched sample. Second, anecdotal evidence suggests that sell-side analysts are highly geographically mobile. For instance, "A typical hard-charging analyst also spends approximately one-third of his or her time traveling, often on marathon trips to multiple cities and hard-to-reach corporate headquarters. Eighteen-hour days are routine. Analysts make dozens of phone calls a day to sources and clients. And they work under extreme pressure." (Groysberg (2010, p.36)). In addition, my recollection of a discussion with two ex-analysts in a prestigious investment bank also indicates that Wall Street analysts generally maintain close contact with their colleagues in other branches through emails or telephone calls. As such, the concern about location proximity is valid, but it should have minimal impact.

subsample of large brokerage houses. This is because large houses are more likely to have branches than small rivals. In contrast, if the results are not driven by the concern of location proximity, the results should be similar based on this subsample of large brokerage houses.

The estimated results are summarized in Figure 3.5. The left (right) panel reports subsample results based on large houses with at least 25 (50) analysts.[60] In general, the results are fairly similar to those reported above, except for *Earnings Forecast Optimism* displayed in green squares. This is not entirely surprising as prior studies show that analysts in large brokerage houses tend to make more accurate earnings forecasts due to a rich information or resource environment (e.g., Clement (1999)). As such, sub-sampling only large houses effectively removes much of the variation in *Earnings Forecast Optimism* by analysts in small brokerage houses. In short, while location proximity is a valid concern, the results reported above are not driven by this potential concern.

3.3.6.5 Other Tests: Diffusion of Early Influence, Absolute Optimism, and Gender

The results of several unreported tests are summarized as follows. To examine whether past influence would diffuse through time as analysts become more experienced, I interact year of general experience with mentor optimism in baseline regressions in Tables 3.2 and 3.3. As the signs of the estimated coefficient on this interaction term are mixed and weak, the results suggest that past mentorship experience is persistent in influencing analyst optimism without a clear trend of diffusion. This is consistent with the finding in Malmendier and Nagel (2011a), who show that, while individuals tend to

[60] The 25-analyst threshold is motivated by the raw data. Specifically, the maximum number of analysts who work in brokerage houses that do not maintain any branches is 26 analysts.

place more weight on recent stock market experience, the "memory of these early experience fades away only slowly" (p.391).

I also examine whether mentors influence analysts' *absolute* earnings forecast optimism (i.e., optimism earnings forecasts to the actual earnings but not relative to peers). Specifically, I replace *Earnings Forecast Optimism* with *Absolute Earnings Forecast Optimism* and run the identical baseline specifications. The estimated coefficient on *Absolute Earnings Forecast Optimism* is 0.911% (*t*-stat: 1.05). It is not entirely surprising due to the underlying construct of absolute optimism: analysts can all be "optimistic" as long as their earnings forecasts are above realized earnings. In short, the evidence suggests that relative but not absolute optimism is influenced by the optimism of past mentors.

I test whether all-star analysts are less influenced by their mentors. Specifically, I interact all-star analyst status in *Institutional Investor* magazines with mentor optimism and run the baseline regressions with this extra interaction term.[61] In a related vein, I also track those mentors who have ever been ranked as all-star analysts and interact this data with mentor optimism. While these conjectures are intuitive, the results indicate that there is no evidence suggesting that all-star analysts would be differently affected by their mentor optimism in their early careers, regardless of whether or not their mentors are all-star analysts.

Motivated by a recent study by Kumar (2009), who shows that female analysts are more accurate in the male-dominated analyst industry, I also test whether there is any gender-related differential of optimism spillover. Specifically, using the manually

[61] In particular, I look into analysts before or after they are elected as all-star analysts. I also examine analysts who have ever been ranked as all-star analysts.

collected gender data on sell-side analysts,[62] I find some weak evidence indicating that a high proportion of female mentors in analysts' early careers would partially unwind the pattern of optimism spillover.[63] However, while the signs of all estimated coefficients are unanimously negative, only one of the four optimism metrics is statistically significant. I also interact female dummy with female mentor dummy/numeric variable to examine any differential in optimism spillover. However, the results are again weak and mixed. As such, there is no consistent evidence that the gender composition of mentors would cast any significant effect on the optimism spillover results documented above.

3.4 Conclusion

This chapter investigates whether analysts' past experience influences their professional careers. The key conjecture of the mentorship experience hypothesis is that analyst optimism should exhibit a systematic relationship to the optimism of the mentors with whom protégés work in their early careers. Analysts are expected to be more optimistic if they work with optimistic mentors. The key innovation is to use mentor optimism, which is a snapshot taken of the analysts' first job in the industry, as an instrument to examine the influence of past mentorship experience on analysts' future optimism. If protégés are influenced by their mentors in their early careers, there should be a positive and systematic relationship between past mentor optimism and future analyst optimism.

[62] My thanks to Alok Kumar for sharing the analyst-gender data.

[63] Specifically, the estimated coefficients on the proportion of female mentors are as follows: *Strong Buys* (-2.972%, *t*-stat: -2.31), *Upgrade Jumps* (-1.736%, *t*-stat:-1.24), *Earnings Forecast Optimism* (-0.922%, *t*-stat: -0.67), and *Price Growth Optimism* (-0.929%, *t*-stat: -0.44).

I find evidence suggesting that past mentorship experience has a strong and visible influence on analysts' professional styles. Analysts are more optimistic if they first work with optimistic analysts in their early careers: they are more optimistic in earnings forecasts and price targets, issue more strong buy recommendations and upgrade jumps. Analysts are also more likely to be influenced by their optimistic mentors than by pessimistic ones.

While it is easy for analysts to pick up their mentors' styles, it is harder for them to learn the skills of their mentors, as there is no spillover in forecast accuracy. Only talented superstar mentors are exceptions to this pattern, passing their superior skills and reputation to their protégés. The market is smart in identifying the protégés of optimistic mentors given that the short-run market reaction to their revisions is weaker.

Collectively, these results suggest that early mentorship experience has a strong and long-lasting influence on analysts.

CHAPTER 4: MARKET REACTION, SMART INSTITUTIONAL INVESTORS, AND SELF SELECTION TESTS

4.1 Introduction

While the above sections show evidence suggesting a strong spillover in optimism, it remains unclear as to whether investors would care about the visible influence of mentors on their protégés. Even if they do care, it also not entirely clear as to whether the market is capable of identifying optimistic analysts based on their early career experience. In this section, I directly address these questions by examining whether the market is smart in identifying the protégés of optimistic analysts (*H2*).

Moreover, the central question now boils down to whether the systematic pattern in optimism spillover is driven by either selection effect or treatment effect. If the pattern described above is explained by selection effect, the results would be interpreted as selection of optimistic analysts into optimistic brokerage houses in their first jobs. On the other hand, if the results are explained by treatment effect, the results would then support the hypothesis that early mentorship experience has a long-lasting influence on analysts' future professional optimism. Without a doubt, the perfect scenario to gauge the economic impact of treatment effect would involve a random assignment of identical equity analysts to random groups and measure their future optimism down the road.[64] Given that such a natural experiment is generally absent in the Wall Street analyst profession, this chapter proceeds forward to establish the causal link between early mentorship experience and future professional styles via the adoption of a host of identification strategies and empirical exercises.

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[64] For instance, economic studies rely on random assignment of college roommates (Sacerdote (2001), Zimmerman (2003)) to estimate the economic impact of peer effect. More specifically, natural experiments refer to "situations where the forces of nature or government policy have conspired to produce an environment somewhat akin to a randomized experiment" (Angrist and Krueger (2001)).

The rest of this chapter is organized as follows. Section 4.2 summarizes the results on market reaction to the forecast revisions made by the protégés of optimistic mentors. Section 4.3 provides further robustness checks showing that the results are not driven by analyst self-selection. Section 4.4 concludes this chapter.

4.2 Smart Market

Following Gleason and Lee (2003), I run the following regression specification to examine the market reaction to earnings revision:[65]

$$\begin{aligned} Adj.\ Return_{i,j,t} &= \alpha + \beta_1 Optimistic\ Mentors_i \times Revision\ Signal_{i,j,t} \\ &+ \beta_2 Optimistic\ Mentors_i + \beta_3 Revision\ Signal_{i,j,t} + \beta_4 X + \beta_5 Fixed\ Effects + e_{i,t} \end{aligned} \tag{4}$$

The dependent variable, *Adj. Return*$_{i,j,t}$, refers to the size-adjusted cumulative return to the revision made by analyst *i* for firm *j* at time *t* in the windows of [-1,+1], [-1,+3], [-1,+5], or [-1,+10].[66] Similar to Table 3.2, *Optimistic Mentors* is a dummy variable that takes one when *Mentor Optimism* in *Earnings Forecast Optimism* is in the top decile in a given year.[67] *Revision Signal* is a discrete variable that takes the value of +1 (-1) if an analyst's revision is both above (below) his or her own prior forecast and the prior consensus for firm *j*, and zero otherwise. The main variable of interest is the interaction term of *Optimistic Mentors* × *Revision Signal*. The primary hypothesis is that, if the market cares about the visible influence of mentors on their protégés and is capable of identifying the protégés of optimistic mentors, this interaction term should be negative.

[65] This regression specification is similar to those in Jiang, Kumar, and Law (2011). The dataset is also drawn from their study.
[66] The size-adjusted cumulative return is the buy-and-hold return of firm *j* for which the revision is made minus the buy-and-hold return for an equal-weighted portfolio of firms in the same NYSE size decile formed at the beginning of each year.
[67] I will also describe results based on an alternative definition of the top quintile.

In contrast, if the market is not able to or does not differentiate analysts based on their past mentors, we should observe no differential in this interaction term.

Similar to those regressions in prior sections, this regression includes a wide set of control variables to control for analyst characteristics and differences in information environment. Specifically, the control variables include all-star dummy, book-to-market ratio, brokerage size, days since last forecast, days to year-end, forecast frequency, firm size, firm-specific experience, general experience, institutional ownership, lag accuracy, 6-month momentum, number of analysts, number of companies, and number of industries. Moreover, all regressions control for time and firm-specific variations by including year and firm fixed effects.

The market reaction results are reported in Table 4.1.[68] The results indicate that the market generally responds more weakly to the revisions made by analysts who have optimistic mentors. All estimated coefficients on the interaction term, *Optimistic Mentors* × *Revision Signal*, are significantly negative. The estimated coefficients range from -0.257% (*t*-stat: -1.98, Column (2)) to -0.489% (*t*-stat: -4.10, Column (1)), depending on regression specifications and control variables. In economic terms, the market discounts the revisions made by the protégés of optimistic mentors by 15-23 basis points. When constructing the optimistic mentor dummy, using an alternative definition based on the top quintile slightly weakens the results (in the last two rows of Panel A). In Panel B, where the pessimistic mentor dummy is included, there is no evidence that the market discounts their revisions since the interaction term *Pessimistic Mentors* × *Revision Signal* are negative but not statistically significant. In short, the market is smart enough to identify the analysts of extremely optimistic mentors and discounts their revisions

[68] To conserve space, the estimated coefficients of these 19 control variables are summarized in Appendix 5.

accordingly. A potential explanation is that the market in general does not trust the protégés of analysts with extreme views; this interpretation is consistent with the idea of trust in Guiso, Sapienza, and Zingales (2008).

Recent studies show that smart institutional investors take analysts' characteristics and bias into account when incorporating the information in analysts' revisions (Malmendier and Shanthikumar (2007), Hugon and Muslu (2010), Jiang, Kumar, and Law (2011)). Specifically, sophisticated institutional investors demand conservative analysts and tend to unwind the optimism bias embed in earnings forecasts made by optimistic analysts. Motivated by these findings, I examine which group of investors is the main driver for the discounting. To address this question, I first sort all observations into terciles (i.e., high, mid, and low) based on their institutional ownership a quarter prior to the revisions. In Panel C, where these subsample results are reported, the results suggest that institutional investors play a main role in discounting the revisions made by the protégés of optimistic mentors. Specifically, the estimated coefficients on *Optimistic Mentor Dummy* × *Revision Signal* are all negative and statistically significant across all short-run event windows, ranging from -0.566% (*t*-stat: -2.67) to -0.888 (*t*-stat: -4.20) in the high institutional ownership bin. While the estimated coefficients are negative in mid and low institutional ownership bins, they are not statistically significant. In economic terms, sophisticated institutional investors discount the revisions made by the protégés of optimistic mentors by 33-38 basis points. These results provide clear evidence that sophisticated institution investors substantially discount the revisions made by the protégés of optimistic mentors, regardless of the direction of revisions.

4.3 Treatment Effect vs. Selection Effect

This section summarizes a wide set of falsification and empirical exercises to disentangle selection effect and treatment effect. The tests reported below indicate that the results are not driven by analyst self-selection.

4.3.1 Unobserved Brokerage House Culture and Incentives

There are cross-sectional differences in optimism at the brokerage house level. First, brokerage houses exhibit heterogeneous levels of optimism as they face difference incentives or reputation concerns (Cowen, Groysberg, and Healy (2006)). Second, houses with a firm-specific culture or organization style exhibit distinct levels of optimism (Groysberg, Lee, and Nanda (2008), Cronqvist, Low, and Nilsson (2009), Parson and Titman (2009)). As brokerage house culture and incentives are generally persistent over time, a potential explanation for the above results is that they merely capture this persistent component. To absorb this cross-sectional difference in firm culture or incentives, all regression results in the above sections have controlled for first brokerage house fixed effects. As further shown in robustness checks, results remain robust even after controlling for current brokerage house fixed effects. These results collectively suggest that the optimism spillover documented above cannot be explained by differences in the unobserved brokerage house culture or incentive structures.

4.3.2 Reverse Spillover from Juniors

A potential question is whether the above results merely document a correlation but not causality. In other words, so far there is no clear evidence showing optimism spillover *from* mentors *to* analysts. As such, it is possible that the professional styles of veteran analysts are influenced by junior apprentices (i.e., a reverse causality argument).

To address these concerns, I conduct additional analyses as follows. Instead of identifying the *Mentors* of each analyst, I track down the *Juniors* of the analysts who would be defined as *Mentors*. In contrast to *Mentors*, *Juniors* are those colleagues who work in the same brokerage house (i.e., same house), who cover the same sector (i.e., same team), but have fewer years of professional experience (i.e., rookies) during analysts' early careers. While this identification strategy is clear and intuitive, the immediate downside is that it is harder to find juniors in analysts' early careers due to the pyramid-shaped organization structure. As such, analysts without corresponding *Juniors* in their early careers would automatically be excluded from the final sample. This is especially important as, for instance, analysts may work for only a year in their first brokerage house and leave, whereas the first brokerage house does not fill the vacancy until years later. To get around this problem, I re-design the sample and increase the sample size by including analysts who join the same house in the same year. Similar to mentor optimism, junior optimism is measured in analysts' early careers to preserve comparability. The following augmented specification is run:

$$Analyst\ Optimism_{i,t} = \alpha + \beta_1 Junior\ Optimism_i + \beta_2 Mentor\ Optimism_i + \beta_3 X + \beta_4 Fixed\ Effects + e_{i,t} \quad (5)$$

If there is a reverse spillover in which optimism spills from junior to senior analysts, we should observe a strong and positive coefficient on β_1. In contrast, if analysts are not influenced by junior analysts in their early careers, we should not observe any systematic differences in β_1.

The above specification also permits us to address the self-selection hypothesis. The idea is as follow: if the results on optimism spillover are driven by self-selection, optimistic juniors should also self-select themselves into optimistic brokerage houses in their first jobs. If this is the case, the estimated coefficient on *Junior Optimism* should be significantly positive. If self-selection cannot explain the systematic spillover in optimism between *Analyst Optimism* and *Mentor Optimism*, we should again not observe any systematic difference in β_1.

Panel C of Table 4.2 reports the regression estimates of *Junior Optimism*. The first subpanel reports the baseline regression results where analysts who join the same house in the same year are counted as *Juniors*, whereas the second subpanel excludes analysts who join the same house in the same year. None of the eight *Junior Optimism* regressions offer significant evidence that there is a reverse spillover from young to senior analysts. In both panels, the estimated coefficients on *Junior Optimism* are not statistically significant in any of the four baseline regressions. While a potential objection is that the power of the test may be weakened because the sample size substantially shrinks due to the identification of juniors, the estimated coefficients on *Mentor Optimism* are reasonably robust. First, in four out the eight regressions the estimated coefficients on *Mentor Optimism* are positive and statistically significant. Second, in all eight regressions the signs of *Mentor Optimism* are positive, which stands in contrast to the estimates of *Junior Optimism*.[69]

[69] The average correlation between junior and mentor optimism is 0.381, whereas the average correlation between junior and analyst optimism is 0.130. Moreover, in an unreported test, I further relax the construction of *Earnings Forecast Optimism* and deviate from Hong and Kubik's (2003) approach by not requiring *Earnings Forecast Optimism* to be computed on a three-year rolling window. Again, there is no evidence of reverse spillover from juniors to senior analysts (estimate: 2.043%, *t*-stat: 1.28).

4.3.3 Span of First Jobs

Motivated by prior economic studies which show that the length of on-the-job experience has a strong economic influence on labor wages (Lynch (1992), Dustmann and Meghir (2005)), I exploit the variation in the length of the first job to examine the influence of mentorship experience. Specifically, if analysts are influenced by their on-the-job learning experience, we should expect a stronger influence from those analysts who stay longer in their first jobs. However, if the results are explained by self-selection, there should not be a systematic difference in optimism spillover depending on the duration of analysts' first jobs.

Panel D of Table 4.2 sorts analysts based on the duration of their first jobs. For analysts who spent two or more years in their first jobs, the results remain robust across different optimism metrics. In contrast, there is no systematic pattern for those analysts who spent one year or less in their first jobs. While *Upgrade Jumps* for this group of analysts remain significantly positive, there is no systematic pattern for the remaining optimism metrics. In short, the results indicate that optimism spillover is stronger for analysts who have a longer tenure in their first jobs and suggest that the results are influenced by their on-the-job learning experience.

4.3.4 Brokerage house sizes

I further exploit the size of initial brokerage houses to isolate between self-selection and treatment effects. The idea is that junior analysts who start their jobs in large brokerage houses simply have more mentors to pick and choose from to begin with. If self-selection is true, the estimated coefficients based on subsamples of brokerage sizes should be stronger (weaker) for larger (smaller) brokerage houses.

Figure 4.1 plots the estimated coefficients of subsamples based on brokerage size. Panel A divides the sample by increments of five analysts (from small houses of five analysts to large houses of 20 analysts). There are two obvious observations. First, the estimated coefficients for large brokerage houses are not necessarily the strongest. Except for *Strong Buys*, the estimated coefficients are stronger for smaller brokerage houses. Second, there is no obvious upward trend in estimated coefficients once we move from small to large brokerage houses (i.e., from left to right). Panel B of Figure 4.1 further compares the smallest (fewer than five analysts) and the biggest brokerage houses (more than fifty analysts). A similar observation is made: the estimated coefficients based on larger brokerage houses are not necessarily stronger. Collectively, these results again suggest that the findings documented above cannot be explained by self-selection.

4.3.5 Other Self-Selection Tests: Mentor Characteristics and Industry Specialization

Potential competing hypotheses are that there is self-selection on observable mentor characteristics or industry specialization. Results reported in Online Appendix 4 and 5 rule out these alternative hypotheses.[70]

In short, approached from different angles, the above results show no evidence of reverse causality; instead, they point to the following two conclusions. First, the evidence suggests a clear economic channel of causality in which protégés are influenced by their senior mentors in their early careers. Second, the results indicate that the systematic pattern shown above cannot be explained by analysts' self-selection in their early careers.

[70] Running baseline regressions with mentor fixed effects to control for unobservable time-invariant mentor characteristics have also been examined. As much of the variation in analyst optimism is absorbed by the high dimensional mentor fixed effects (e.g., 1,000-2,000 mentor dummies) and the main sample at analyst-year level is not large to begin with, the results are expectedly much weaker.

4.4 Conclusion

I first show that the market generally responds more weakly to the revisions made by analysts who have optimistic mentors by 15-23 basis points within 10-day window. More specifically, sophisticated institutional investors are the main driver for this discounting: they discount the revisions made by the protégés of optimistic mentors by 33-38 basis points within 10-day window. These results provide clear evidence that the market, especially sophisticated investors, cares about the personal characteristics of analysts when digesting their forecast revisions.

Moreover, a wide set of falsification tests and econometric exercise show that these results on optimism spillover cannot be explained by selection of analysts into optimistic brokerage houses in their first jobs. More specifically, competing hypotheses including unobserved brokerage firm culture, common portfolio coverage between mentors and protégés, analysts self-selecting into mentors based on their historical optimism, and industry self-selection cannot explain the spillover. Furthermore, subsamples based on brokerage sizes also show no systematic evidence on self-selection.

In addition to the above, analyses show that there is no reverse spillover in optimism from juniors to their mentors, suggesting a clear causality channel that analysts learn their mentors' styles, but not vice versa. The lack of reverse spillover also suggests that the systematic pattern in optimism spillover cannot be explained by analysts' self-selection in their early careers. Additional tests examining the span of analysts' first jobs also show that the spillover is stronger among those analysts who spend a longer time with their mentors in their first jobs, thereby supporting the conclusion that analysts are influenced by their mentors via social learning.

CHAPTER 5: HARD-TIMES ANALYSTS

5.1 Introduction

An increasing number of economics and finance studies shows that the macroeconomic environment has a long-lasting influence on individuals' future decision making. Schoar and Zuo (2011) find that CEOs who start their first careers in recessions face more difficulty experiencing favorable job separation and adopt more conservative corporate strategies. Oyer (2006; 2008) shows that the stock market condition strongly determines whether MBA graduates get their first jobs on Wall Street. A number of economic studies also show that the initial labor market condition has a long-lasting impact on graduates' future earnings and career paths[71]; however, little is known about whether the style and skills of managers depend on the macroeconomic environment when they first start their manager roles.

In this paper, I use the analyst profession as *a* setting to examine the above hypothesis. Using cyclical labor market as exogenous shocks, this study asks whether sell-side analysts differ in their style and skills depending on cyclical shocks in the labor market when they first start their analyst roles. As managers of their portfolios, young analysts not only start issuing stock recommendations under their own names, they also start developing their own specific human capital through on-the-job learning experience. The first time that analysts put their reputation on the line provides an ideal setting and is a clear career milestone to examine this "first time manager" hypothesis. If the macroeconomic environment has a formative impact on their style and skills, we should

[71] For example, Khan (2006) and Oreopoulos, von Wachter, and Heisz (2011) show that initial labor market conditions have a long-lasting impact on graduates' future earnings and career advancement. In a similar vein, Law (2012) also demonstrates that early career mentors have a long-lasting influence on analysts' future style but not their accuracy. In the personal domain, Malmendier and Nagel (2011a) show that depression-era babies are less willing to take financial risks in the future.

observe a systematic relationship between the macroeconomic condition during their early macroeconomic careers and future performance.

The findings in this paper show an intricate dynamic between early macroeconomic experience and analysts' long-term style and skills. First, analysts who start their careers in recession times (i.e., recession analysts) are more pessimistic in their forecasts than non-recession cohorts by 3.147%-4.335% in standardized forecast optimism, which is not economically trivial when compared to other known determinants identified in the prior literature. Second, their forecast accuracy is lower than non-recession cohorts by 4.176%-7.006% in standardized forecast accuracy. These results persist after controlling for a wide host of known variables that affect analyst performance and remain robust under a series of robustness tests. Moreover, as a result of low forecast accuracy, recession analysts are less likely than their non-recession cohorts to become all-star analysts in the future by about 2%. On the contrary, while boom analysts are shown to be more optimistic in their future careers, they are not necessarily more accurate in their forecasting. Moreover, the optimism of boom analysts is short-lived and, unlike recession analysts, reverses in the short run.

This paper makes several contributions to the existing literature. First, the evidence suggests that the initial labor market condition has a long-lasting impact on analysts' future performance and style. Understanding the impact of analysts' formative experience on their information dissemination is important, as it helps us to identify the main economic determinants of their style, performance, and belief formation. Second, these findings complement Schoar and Zuo (2011), as the identification strategy in this paper exploits the actual timing when analysts first assume their analyst roles. Unlike typical cohort studies, the results in this paper are unlikely to be driven by the endogenous timing of graduation, as first-time analysts have been working for years before they begin coverage of their own companies.

The evidence in this study also sheds light on the interaction between firm-specific human capital development and labor market dynamics. As analysts work for a number of years before becoming promoted to lead analysts, the above findings suggest that firm-specific promotion dynamics are endogenous to time-varying business cycles. The type of analyst being promoted is endogenously determined based on the prevailing labor market conditions. Pessimistic (Optimistic) analysts are more likely to be promoted in bad (good) economic times. Prior studies have suggested that sell-side analysts bow to pressure, as different brokerage houses have different incentive structures or cultures, and analysts face different career concerns depending on the stage of their career. The finding that recession analysts hold onto their pessimistic view and do not exhibit any reversal in future careers suggests that there is an unobserved characteristic among recession analysts. Comparing this evidence with the finding that the optimism of boom analysts is short-lived, these results collectively indicate an unobserved heterogeneous difference in belief formation between these two types of cohorts. Moreover, these findings echo the general consensus in economic studies, where recession experience is shown to leave a more remarkable imprint on individuals than boom experience (e.g., Malmendier and Nagel (2011a, 2011b)).

The asymmetric impact of early macroeconomic experience on forecast accuracy between boom and recession analysts also points to another unobserved heterogeneity in information dissemination across these two groups. More specifically, the fact that recession analysts hold onto their pessimistic views has a detrimental impact on their future performance. On the other hand, as boom analysts are able to adjust or unwind their initial optimism over time, their early labor market condition does not seem to have any long-lasting impact on their future skills development.

This paper is related to a number of recent papers analyzing the impact of past experience on individuals' future decision makings. For example, studies have examined corporate financial policies (Graham and Narasimhan (2004), Graham, Harvey, and Puri (2010)), 401(k) savings (Choi, Laibson, Madrian, Metrick (2009)), inflation expectation (Malmendier and Nagel (2011b)), stock market or IPO entry decisions (Kaustia and Knüpfer (2008), Chiang, Hirshleifer, Qian, and Sherman (2011)), investment and savings decisions (Puri and Robinson (2007), Osili and Paulson (2008)), aggregate consumption (Alesina and Fuchs-Schündeln (2007)), trust (Guiso, Sapienza, and Zingales (2008)), and the investment strategies of mutual fund managers (Greenwood and Nagel (2009)).

The closest study to this paper is Khang (2011). The primary difference is that Khang (2011) focuses on the impact of recession on accuracy. Using quantile regression, Khang shows that recession cohorts are *more* accurate at certain quantiles in their first and the third years, but there is no differential in analysts' accuracy in the long-run.

The rest of the paper is organized as follows. Section 5.2 summarizes the sample data and methodology. Section 5.3 discusses the main results. Conclusions are in Section 5.4.

5.2 Sample and Methodology

All earnings per share (EPS) forecasts are obtained from Thomson Reuter's IBES dataset. All earnings forecasts used in the study are based on unadjusted earnings forecasts between 1983 and 2010. As prior studies suggest that the coverage in the IBES database is thin and incomplete prior to 1983, I focus on those analysts who first appear in IBES on or after 1984 (O'Brien (1988), Zitzewitz (2001)). Following Clement (1999) and Hong and Kubik (2003), the sample focuses on the last firm-year forecasts issued by analysts at least 30 days, but not more than one year, before the fiscal year-end. All forecasts and recommendations are merged with the Center for Research on Security Prices to obtain relevant stock prices. Consistent with prior studies, an analyst first starts his or her career when the analyst's first earnings forecast is recorded in IBES.

Following Hong, Kubik, and Solomon (2000) and Hong and Kubik (2003), the main metric on forecast optimism is the percentile-rank optimism. An analyst's *Earnings Forecast Optimism* on stock *j* at time *t* is calculated as follows:

$$Forecast\ optimism_{i,j,t} = 100 - \left[\left(\frac{Rank\ of\ forecast\ error_{i,j,t}\ -\ 1}{Number\ of\ analysts\ following\ stock\ j\ -\ 1} \right) \times 100 \right] \tag{6}$$

Rank of forecast error is a rank variable based on the rank of an analyst's forecast error depending on one-year-ahead EPS forecasts. The most (least) optimistic analyst covering stock *j* in time *t* would be given the first (last) rank. The most optimistic (pessimistic) analyst would score 100 (0) in *Forecast optimism*. As this metric is conditional on the same firm-year, this is identical to controlling for firm-year fixed effects. Following Hong and Kubik (2003), an analyst must have at least three years of experience prior to being included in the sample, and the above measure is calculated on a three-year rolling window. Moreover, analysts are also required to cover at least three firms in any given year to ensure that the results are not driven by extreme values, as is commonly observed across young analysts who have thin portfolio coverage.

Similar to the above metric, *Forecast accuracy* is relative (to analyst peers) in nature, where the most accurate analyst covering stock *j* in time *t* would be given the first (last) rank.

$$Forecast\ accuracy_{i,j,t} = 100 - \left[\left(\frac{Rank\ of\ forecast\ error_{i,j,t}\ -\ 1}{Number\ of\ analysts\ following\ stock\ j\ -\ 1} \right) \times 100 \right] \tag{7}$$

In other words, the most (least) accurate analysts would score 100 (0) in *Forecast accuracy*.[72]

5.3 Main Results

In this section, I first provide a brief descriptive statistics summary, followed by the main results.

5.3.1 Brief Statistics Summary

Table 5.1 reports the summary statistics of the main variables in this study. Approximately 22% of first-time analysts start their jobs in hard times. Recession analysts are more pessimistic (0.69 percentile; *t*-stat: 3.64) and less accurate (1.12 percentile; *t*-stat: 5.80). They are 2% less likely to ever become all-star analysts, cover more companies, issue forecasts closer to fiscal year-ends, and cover stocks with less

[72] Hong and Kubik (2003) show that from 1983 to 2000 an analyst's relative optimism and accuracy are negatively correlated (about -0.18), which I am able to replicate (correlation: -0.162); however, an analyst's relative optimism and accuracy are not necessarily negatively correlated by design. A simple example would illustrate this point. Assume two analysts (A and B) make forecast errors of +0.5 and -0.5, respectively. While they both score 100 in percentile-rank accuracy, analyst A (B) would score 100 (0) in percentile-rank optimism. In other words, an analyst's relative optimism and accuracy are not necessarily a hard-wired relationship. Raw data also suggest a similar conjecture, as the correlation of optimism and accuracy is positive (correlation: 0.0783) from 2001 to 2010.

analyst coverage. More specifically, recession cohorts usually start their jobs in larger brokerage houses (5 more analysts in their first jobs; *t*-stat: 3.94) and move to smaller brokerage houses in their subsequent careers.

5.3.2 Baseline Specifications

To examine whether analyst are influenced by their early labor market experience, I run the following baseline panel regression:

$$Forecast\ Optimism_{i,t} = \alpha + \beta_1 Recsseion_i + \beta_2 X + \beta_3 Fixed\ Effects + e_{i,t} \tag{8}$$

For each analyst *i* in year *t*, I regress the analyst *i*'s forecast optimism on *Recession* dummy, a *k*-vector a (K×1) of control variables (X), and a set of fixed effects controlling for differences in information environments. *Recession* is an indicator that takes one when an analyst first appears in the IBES dataset as a lead analyst during the recession years. The main coefficient of interest is β_1 as it captures the influence of the early labor market's influence on analysts' future styles and performance.

A similar regression specification is run to determine analysts' forecast accuracy:

$$Forecast\ Accuracy_{i,t} = \alpha + \beta_1 Recsseion_i + \beta_2 X + \beta_3 Fixed\ Effects + e_{i,t} \tag{9}$$

X is a wide selection of control variables including analyst characteristics or other variables that have been shown in prior literature to have an influence on forecast optimism and accuracy. They include number of companies, number of industries, general experience, firm-specific experience, all-star analyst status, the proportion of affiliated clients (*Underwriting*), days to year-end (O'Brien (1988), Klein (1990), Clement (1999), Clement and Tse (2005)), and firm coverage (i.e., the average analyst coverage of an analyst's portfolio of firms) (Hong and Kubik (2003)). To address the concern that optimism metrics are likely to be correlated within analysts, all standard errors are clustered at either analyst-year or analyst level.

5.3.3 Analyst Forecast Optimism and Accuracy

The baseline results on forecast optimism are reported in Table 5.2.

In Table 5.2, the main dependent variable is *Forecast Optimism*. Across all specifications, there is a strong systematic pattern showing that recession analysts are more pessimistic than their peers in their future professional careers while controlling for other known factors. The coefficients are all significantly negative, ranging from -3.147% (*t*-stat: -2.61) to -4.335% (*t*-stat:-3.64), depending on regression specifications.[73] As the dependent and independent variables (except dummy) have been standardized with zero mean zero and unit standard deviation, it is easy to interpret the economic magnitudes of these estimated coefficients: recession analysts are more pessimistic than non-recession cohorts by 3.147%-4.335% in standardized *Forecast Optimism*, which is not economically small when compared to other known determinants.

The baseline results on forecast accuracy are reported in Table 5.3. The main dependent variable is *Forecast Accuracy*. The estimated coefficients on *Recession* across all regression specifications are statistically negative, ranging from -4.176 (-4.04) to -7.006 (-5.80). In other words, recession cohorts are less accurate than non-recession cohorts by 4.176%-7.006% in standardized *Forecast Accuracy*, depending on the

[73] The results in Tables 5.2 and 5.3 are not affected by the computation of standard error. Standard errors are similar but larger if they are duly clustered at year and analyst level. From untabulated results, the estimated coefficients on *Forecast Optimism* range from -3.147% (*t*-stat: -2.08) to -4.335% (*t*-stat: -2.44), whereas the estimated coefficients on *Forecast Accuracy* range from -5.458% (*t*-stat: -3.83) to -7.006% (*t*-stat: -3.06).

regression specifications. The absolute magnitude of *Recession* is approximately ranked as the third (fifth) most important determinant in explaining the variation in *Forecast Optimism* (*Forecast Accuracy*) in column 5 when different known variables are controlled for.[74,75]

Table 5.4 reports the results of different robustness checks. The above results survive an extensive list of robustness checks.

After 1993 excludes forecasts made before the year 1993. *After Regulation FD* uses sample data on or after year 2001, the year after the Regulation Fair Disclosure was enacted. *Left Censored at 1984* excludes those analysts who first appear in the IBES before 1984. *At Least 3 Years of Experience* includes those analysts who have at least three years of experience. *Controlling for Industry-Year FEs* controls for year-industry fixed effects (instead of year and industry fixed effects) in baseline regressions. *Price-Scaled Dependent Variables* replace the dependent variables with (absolute) forecast error variables relative to actual earnings, scaled by lag price in the prior month. *Non-Linear Control Variables* replace *Firm-Specific Experience*, *General Experience*, *Number of Companies/Industries*, *Brokerage Size*, *Days to Year-End*, and *Firm Coverage* with the natural logarithm of the corresponding variables of interest. *FM Regressions* report the regression results estimated using annual time-series cross-sectional Fama-MacBeth (1973) regressions. *NBER recession* uses an alternative definition and replaces

[74] Brokerage house size has a strong influence on *Forecast Optimism* and *Forecast Accuracy*. In untabulated results where I re-estimate specification (5) in Tables 5.2 and 5.3 without brokerage house fixed effects, the estimated coefficients on brokerage house size are -2.160 (*t*-stat:-4.19) and 4.699 (*t*-stat:10.25), respectively. The corresponding estimated coefficients on *Recession* are -3.216 (*t*-stat:-2.69) and -4.737 (*t*-stat:-4.55), respectively. The explanatory power of brokerage house size is currently subsumed by brokerage house fixed effect in Tables 5.2 and 5.3.

[75] The statistical significance of *Days to year-end* would substantially decrease once the standard errors are clustered at the time dimension. In untabulated results, when I re-estimate specifications (5) in Tables 5.2 and 5.3 with standard errors clustered at time level, the statistical significances for *Days to year-end* are 4.84 and -21.01, respectively.

Recession with *NBER recession*, which takes one when analysts first appear in IBES during the NBER recessions. The results are much weaker under specifications (1) and (2) but are similar under specifications (3)-(5) where more control variables are included. The estimated coefficients on *Recession* after re-estimating those regression specifications in Tables 5.2 and 5.3 range from -1.618 (*t*-stat:-1.76) to -9.689 (*t*-stat:-4.83).

Overall, the above evidence suggests that recession analysts are more pessimistic and less accurate than their peers.

5.3.4 Diffusion of Experience

Table 5.5 examines whether analyst pessimism and accuracy would diffuse as analysts accumulate more experience in the industry profession.

Panel A re-estimates the baseline regressions by including an interaction between *Young Analyst*, which takes one during the first three years of an analyst's career, and *Recession*. First, young analysts appear to be more pessimistic during their early careers, as the estimated coefficients on *Young Analyst* are all negative, ranging from -0.590 (*t*-stat:-0.54) to -4.302 (*t*-stat:-3.64). The pessimism of recession analysts persists over time, as the estimated coefficients on the interaction of *Recession* and *Young Analyst* are all positive but not statistically significant across different specifications from columns (1) to (5). For *Forecast Accuracy*, recession analysts are less accurate during their early careers (than their later careers), as the estimated coefficients on the interaction of *Recession* and *Young Analyst* are all statistically significant and negative, ranging from -4.755 (*t*-stat:-2.40) to -6.979 (*t*-stat:-3.16). In other words, recession analysts are more inaccurate (than non-recession peers) during their whole careers, and they are more inaccurate in their early careers (than in their later careers).

Panel B includes an interaction of *Recession* and *General experience* to capture the diffusion of experience on analysts' forecast optimism and accuracy. The estimated coefficients on the interaction term do not show any systematic difference on analyst optimism, as the negative coefficients are not statistically significant across different specifications. For *Forecast Accuracy*, recession analysts tend to improve their accuracy when they have longer professional experience. As various variables (except dummy) have been standardized, the results in Panel B suggest that the inaccurate performance of recession analysts tends to dissipate after recession analysts spend 2-3 years in the profession. Similar to Panel A, these results suggest that (1) recession analysts tend to perform worse in their early careers (than in their later careers), but (2) this discrepancy tends to decrease a few years down the road. On the other hand, how many years recession analysts spend in the profession has no influence on their pessimism, suggesting again that the pessimism of recession analysts tends to persist over time.

5.3.5 Boom and Recession Analysts

I further examine whether analysts who first become lead analysts in boom times are more optimistic than their non-boom peers. More specifically, I define *Boom* as a dummy that takes one when an analyst first appears in IBES during the years when the non-negative annual employment growth in the analyst labor market is in its top tertile, or zero otherwise.

Table 5.6 summarizes the results by including *Boom* in baseline regressions. In Panel A, boom analysts are more optimistic than non-boom analysts, as all estimated coefficients on *Forecast Optimism* are statistically significant and positive (from 3.098% (*t*-stat:2.81) to 3.194% (*t*-stat:2.90)) across different specifications in columns (1)-(5). In Panel B, I further include *Recession* and find that the optimism (pessimism) of boom (recession) analysts continues to survive (except for *Boom* in specification (2)). Recession analysts are more influenced by the initial labor market condition than boom analysts, as the absolute values of the estimated coefficients on *Recession* are larger than those on *Boom*. Panel C further reports the robustness checks of the above results. I re-estimate the regressions following those robustness checks in Table 5.4. Except for *3 Years* and *Price-Scaled Dependent Variable*, where the estimated coefficients are not significant, all other estimated coefficients are significantly positive. The insignificance of these two tests suggests that (1) the optimism of boom analysts tends to be short-lived and dissipates after their early careers (i.e., after 3 years), and (2) boom analysts are only more optimistic relative to their peers (i.e., percentile-rank optimism metric), but the levels of their forecasts (i.e., price-scaled forecast error) are not necessarily lower than non-recession cohorts.

The results on *Forecast Accuracy* are reported in columns (6)-(10) in Panels A and B. For forecast accuracy in Panel A, boom analysts seem to be more accurate than recession analysts, as the estimated coefficients on *Boom* range from 3.456 (*t*-stat:3.54) to 4.477 (*t*-stat:3.86). In Panel B, its strong economic and statistical significance remains even after *Recession* is included in the baseline regressions. However, these results are less robust, as the estimated coefficients in Panel D are weak and insignificant under a number of robustness tests: *After 1993, After Regulation FD*, *Price-Scaled Dependent Variable, and Fama-MacBeth Regressions*. Overall, boom analysts appear to be more optimistic than their non-boom peers, but it seems that there is only very weak evidence suggesting their superior accuracy.

On the contrary, the estimated coefficients on *Recession* remain robust, as they continue to be strongly negative across different specifications even after *Boom* is included. More specifically, recession analysts are shown to be more pessimistic than their peers in Panels B, C and D, as the test that gives the lowest statistical significance on *Recession* is the one on *3 Years* (i.e., excluding young analysts with less than three years of experience), where -1.68 (estimated coefficient:-3.074) is reported.

5.3.6 All-Star Analysts

To examine the influence of their job performance on attaining all-star analyst status, I run the logistic regressions regressing *Ever Star*, which takes one after an analyst is ranked as an all-star analyst in *Institutional Investor* magazine, on a wide host of known determinants.

Table 5.7 reports the estimated marginal probabilities of these pooled logistic regressions. In Panel A, recession analysts are less likely to become all-star analysts by 1.5% (*t*-stat:1.70) to 2.4% (*t*-stat:2.23). The lower probability of becoming all-star analysts is not surprising, as recession analysts have lower forecast accuracy (notably during their early careers). I re-estimate these logistic regressions on boom analysts in Panels B and C, but do not find any systematic difference in their probabilities of becoming all-star analysts. This confirms the earlier findings in Panel D of Table 5.6, where boom analysts are shown as not necessarily more accurate.

5.4 Conclusion

This paper investigates whether the cyclical labor market has a long-lasting impact on first-time managers' style and performance. Using the analyst profession as a setting, I find that analysts who start their analyst careers in recessions (i.e., recession analysts) are more pessimistic, less accurate, and less likely to become all-star analysts than their non-recession cohorts. These results are robust after controlling for a wide host of known variables that influence analysts' style and skills. On the contrary, while boom analysts are shown to be more optimistic in their future careers, they do not necessarily have greater forecast accuracy.

CHAPTER 6: CONCLUSION

Using the sell-side analyst as a setting, this dissertation examines the following question: how do individuals' past experiences affect their future economic behaviors? More specifically, these chapters comprehensively examine to what extent analysts' styles and skills are influenced by their early mentorship and labor market experience when they first start their analyst jobs.

Chapter 3 shows that early career mentors influence sell-side analysts' optimism and skills. I defined *early career mentors* as those colleagues who work in the same brokerage house (i.e., same house), cover the same sector (i.e., same team), and have more years of professional experience (i.e., veteran) during analysts' early careers as junior analysts. While analysts are more optimistic (pessimistic) in their future professional careers if they work with optimistic (pessimistic) mentors in their early careers, I find that there is no spillover in accuracy. Only talented superstar mentors (i.e., all-star analysts) can break this pattern, spilling their forecasting skills to their protégés.

Chapter 4 indicates that market participants are able to take the mentor-mentee relationship as a personal characteristic into account when digesting analysts' forecast revisions. More specifically, the market generally responds more weakly to the revisions made by analysts who have optimistic mentors. Sophisticated institution investors substantially discount the revisions made by the protégés of optimistic mentors, regardless of the direction of revisions.

In this chapter, I also implement a wide set of falsification tests and econometric exercises showing that the results on optimism spillover are not driven by analysts' self-selection into optimistic brokerage houses or mentor relationships.

Chapter 5 exploits another novel setting and finds the styles and skills of first-time analysts depend on cyclical shocks in the labor market. Using the cyclical labor market as a form of exogenous shock, I exploit a novel setting to examine if sell-side analysts differ in their style and skills depending on their early career macroeconomic environment. This paper shows that analysts who start their careers in a tough labor market are more pessimistic, less accurate, and less likely to become all-star analysts than their non-recession peers. While their forecast accuracy improves as they gain more experience, they continue to hold onto their pessimism. On the contrary, while boom analysts are more optimistic, they are not necessarily more accurate. Moreover, their optimism is shown to be short-lived. Collectively, the above evidence suggests that the development of firm-specific human capital is endogenous to time-varying macroeconomic conditions. The asymmetric performance of boom and recession analysts indicates that there are unobserved heterogeneities in their human capital and information dissemination.

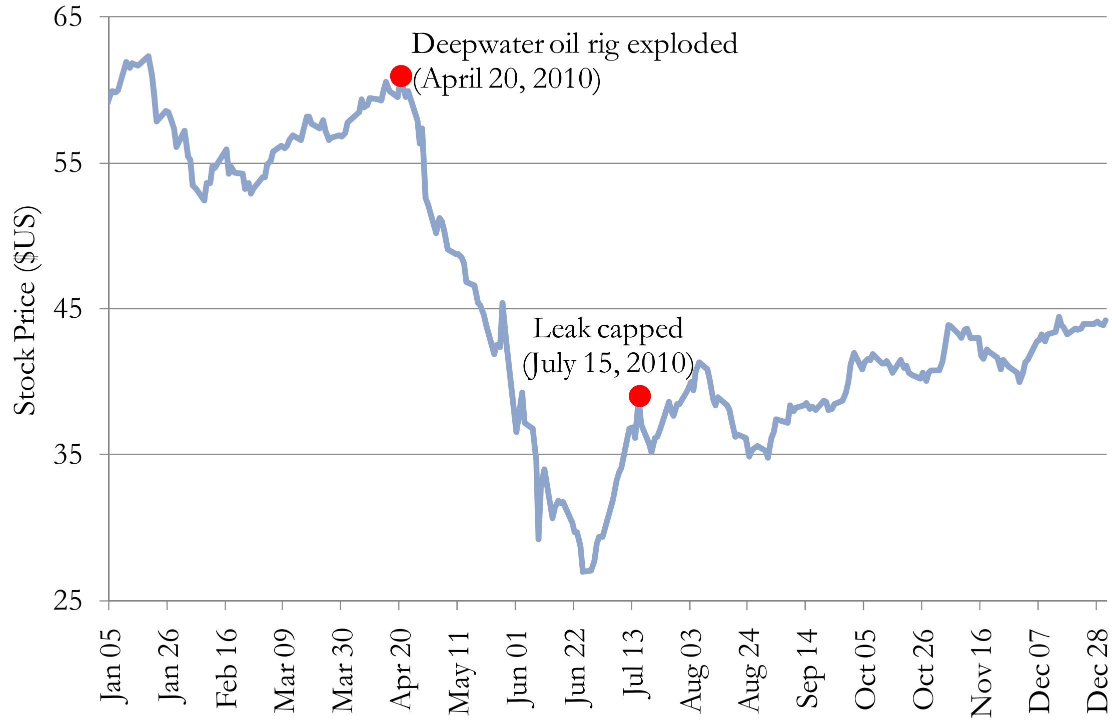


Figure 2.1: BP's Stock Price Movements
The figure above plots the stock price movements of BP from January 1 to December 31, 2010.

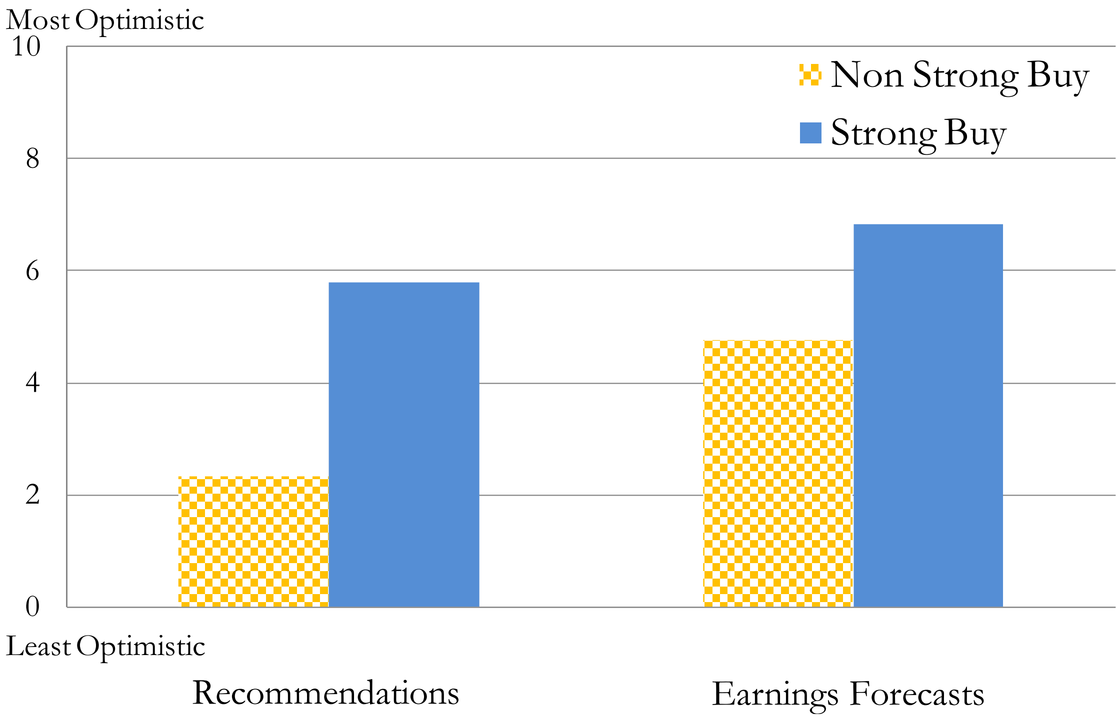


Figure 2.2: Historical Levels of Mentor Optimism
All bars above represent different measures of historical optimism of analysts' mentors during analysts' early careers. "Recommendations" refers to the average proportion of "Strong Buy" recommendations initiated, reiterated, or upgraded in a mentor's portfolio during a given year, averaged across all analyst-mentors pairs. "Earnings Forecasts" refers to the average percentile-rank optimism (Hong, Kubik, and Solomon, 2000; Hong and Kubik, 2003) in one-year-ahead earnings per share forecasts of all mentors during an analyst's early career, averaged across all analyst-mentors pairs. Early career is defined as either an analyst's first job or the first three years of an analyst's first job, whichever is shorter, provided that an analyst stayed for at least six months.

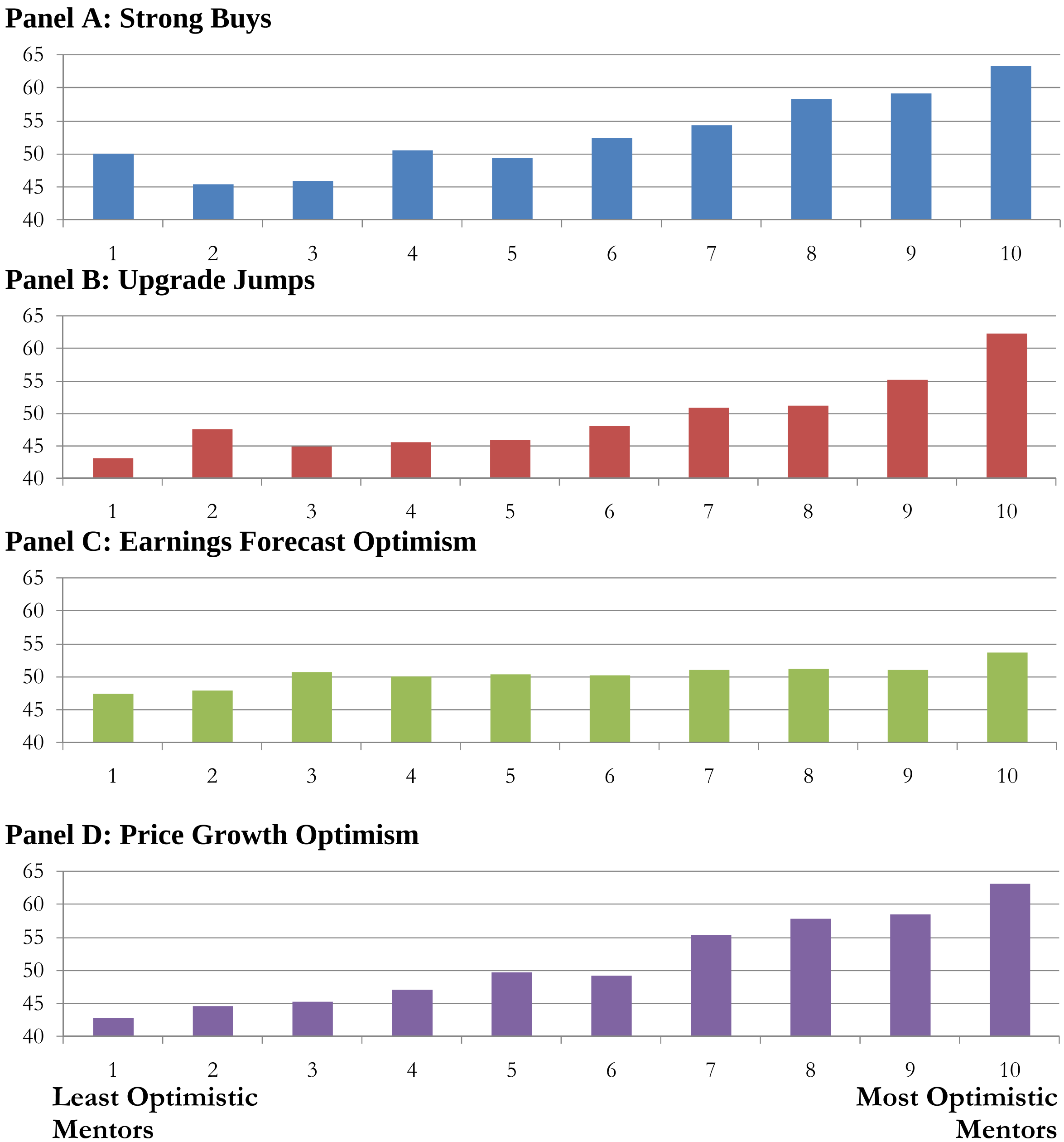


Figure 3.1: Analyst Optimism on Historical Mentor Optimism

These bar charts plot the averages of *Analyst Optimism* (y-axis) on *Mentor Optimism* (x-axis). All *Mentor Optimism* is measured historically based on the optimism of analysts' mentors in the analysts' first jobs. Distributions are created for each figure as follows: *Analyst Optimism* is first assigned a percentile rank score from 0 (least optimistic) to 100 (most optimistic) each year. Each historical *Mentor Optimism* is also sorted into deciles from the least optimistic mentors (bin 1) to the most optimistic mentors (bin 10). For each *Mentor Optimism* decile, *Analyst Optimism* is then estimated and averaged.

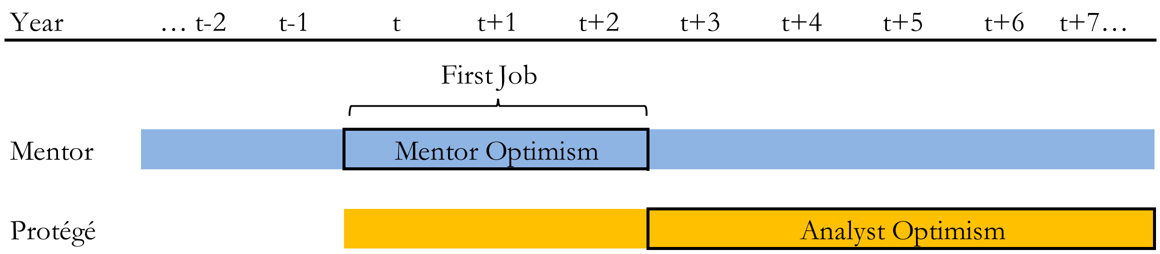


Figure 3.2: Timelines of Mentor Optimism: An Example
The above timelines illustrate the timing of *Mentor Optimism* and *Analyst Optimism*. Analysts' employment history is highlighted in color (blue for mentor; orange for analyst). Assuming that an analyst spends his or her first three years (from years *t* to *t*+2) in a brokerage house with his or her mentors, *Mentor Optimism* is measured based on the optimism of the mentors in the analyst's first brokerage house during years *t* to *t*+2 (bolded box). For *Analyst Optimism* (bolded box), it is only measured after the first job from years *t*+3 and beyond. Note that these optimism metrics are measured sequentially but not simultaneously.

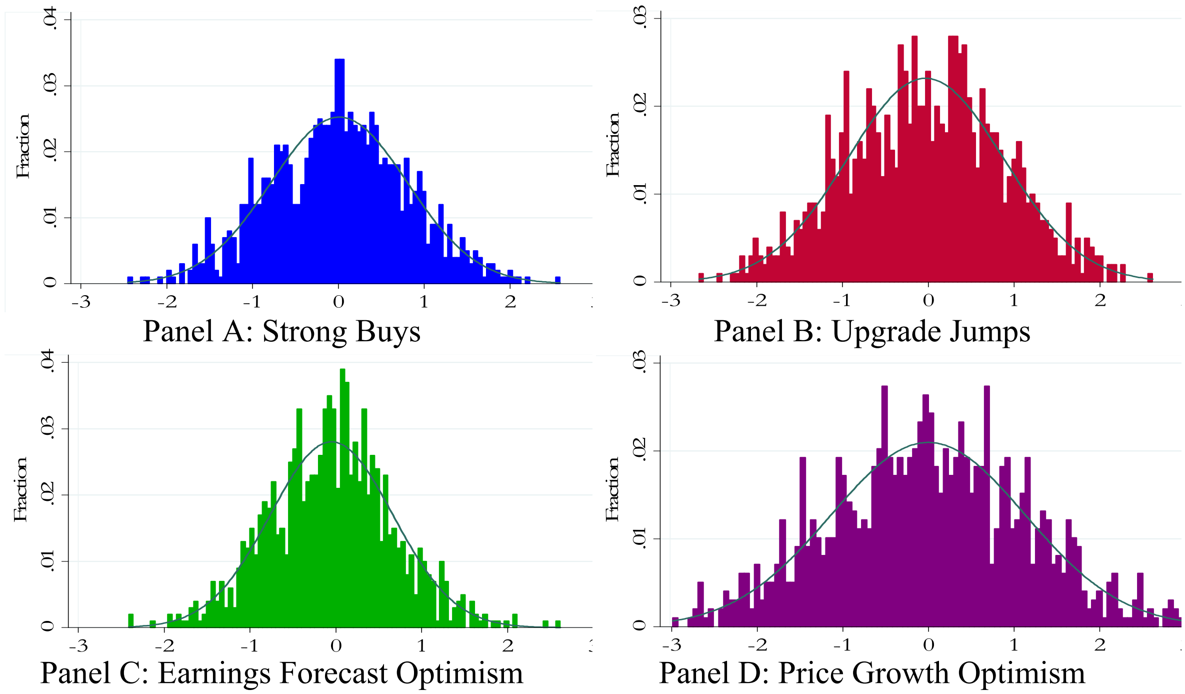


Figure 3.3: Distributions of Estimated Coefficients on *Pseudo-Mentor Optimism*

The figure plots the distributions of estimated coefficients on *Pseudo-Mentor Optimism*. The steps of pseudo-mentor randomization are as follows. First, analysts are matched with mentors randomly drawn from the sample without replacement. The final deliverable is a simulated panel on employment history in which analysts are matched with *Pseudo-Mentors*. Second, after assembling this pseudo-mentor panel, regressions are run with the optimism metrics of the pseudo-mentors (i.e., *Pseudo-Mentor Optimism*) with the control variables and fixed effects identical to Columns (4) and (8) in Tables 3.2 and 3.3, respectively. This two-step randomization is repeated 1,000 times. To improve readability, the estimated coefficients ranged from -3 to +3 have been multiplied by 100. Normal density kernels are also imposed in thin black lines for illustration.

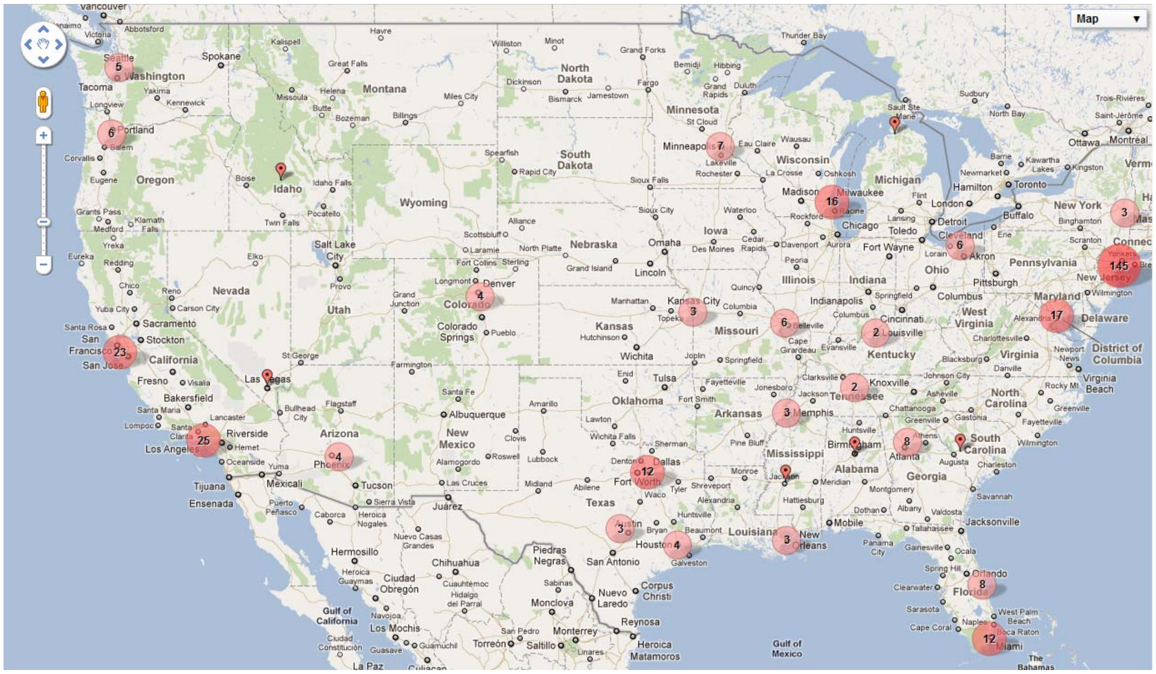


Figure 3.4: 2008 Location Snapshot of Brokerage Houses and Their Branches
This figure is a snapshot of brokerage house location (including all headquarters and branches) at the end of 2008. The number in each circle is the total number of brokerage houses. This snapshot is graphed using Google Maps.

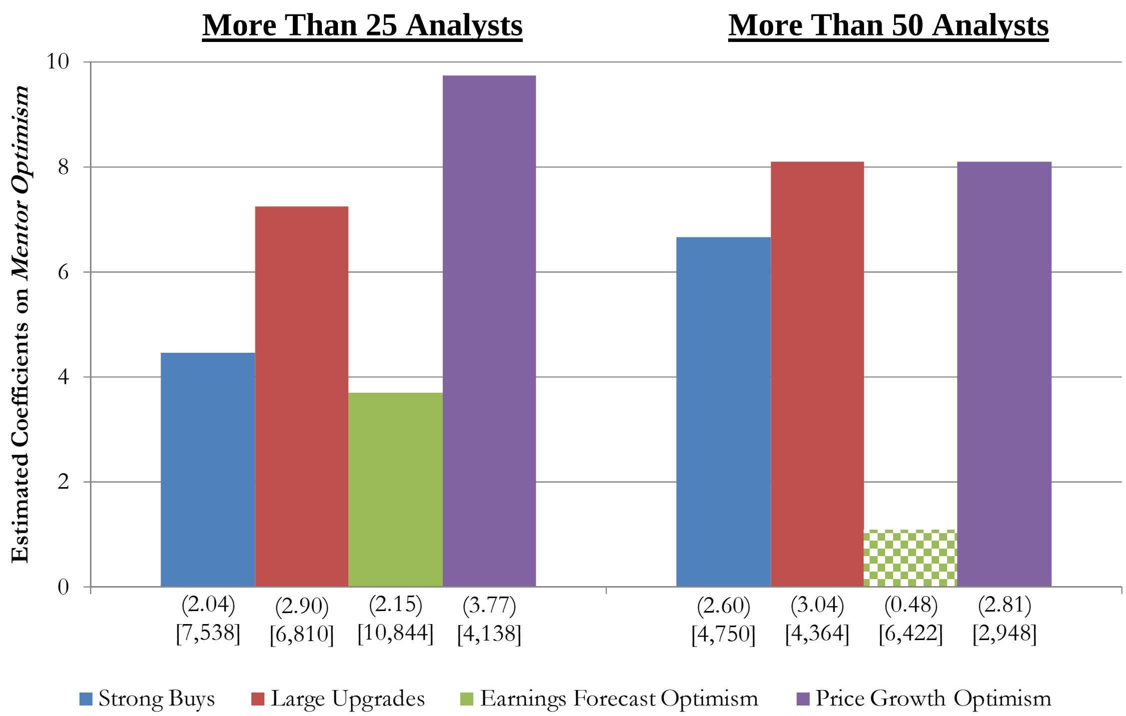
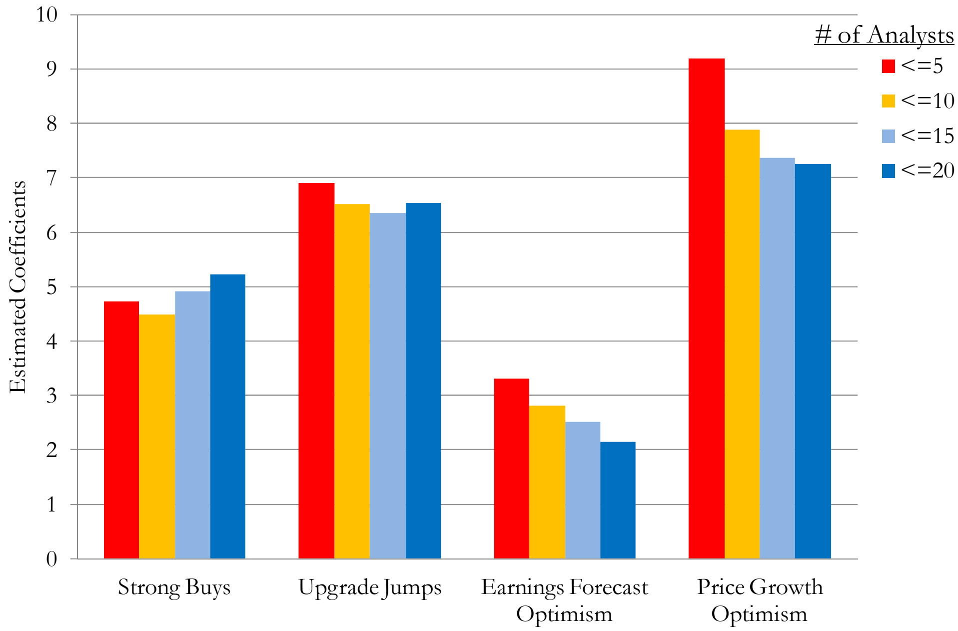


Figure 3.5: Location Proximity Tests

The figure above reports the estimated coefficients on *Mentor Optimism*. The regression specifications are identical to those in Columns (4) and (8) in Tables 3.2 and 3.3. Results in left (right) panel are based on analysts who have more than 25 (50) analysts in their first brokerage houses. *t*-statistics (Number of observations) are reported in parentheses (brackets). All estimates are statistically significant at the 5% level except for *Earnings Forecast Optimism* displayed in green squares.

Panel A: Subsamples based on Brokerage Sizes



Panel B: Subsamples based on Large and Small Brokerage Houses

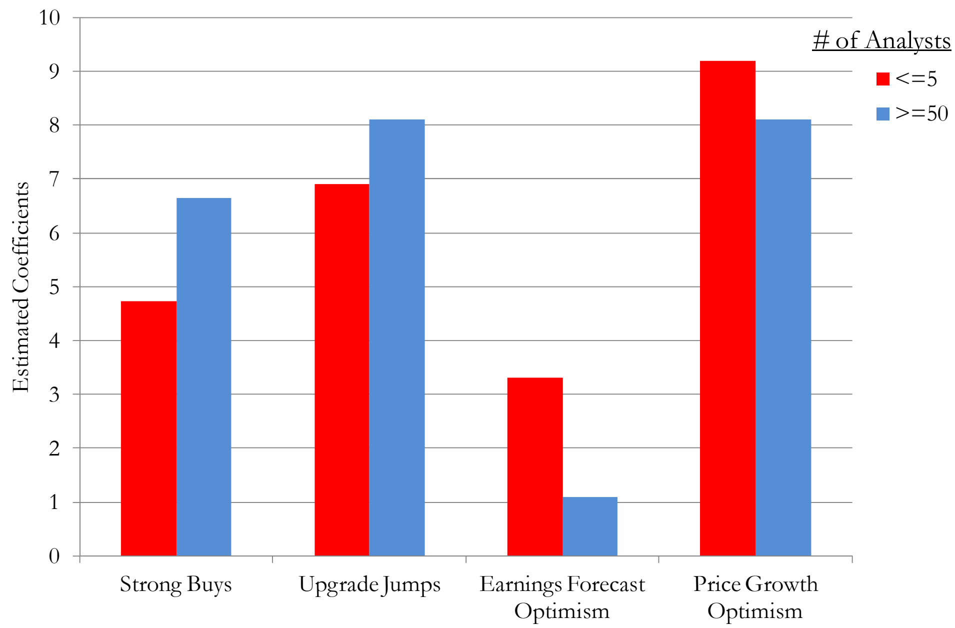


Figure 4.1: Subsamples based on Size of First Brokerage Houses
The above charts plot the estimated coefficients on *Mentor Optimism* based on the size of brokerage houses when analysts first start their jobs.

Table 3.1
Summary Statistics

Panel A provides the summary statistics for the main variables in the sample. Panel B reports the pairwise Pearson correlations. Panel C reports the distribution of analyst-mentor pairs in the main sample. Earnings forecasts, recommendations, and price targets are all obtained from IBES. *Mentors* are those colleagues who work in the same brokerage house (i.e., same house), cover the same IBES sector (i.e., same team), and have more years of professional experience (i.e., veteran) during analysts' early careers as protégés. *Early Career* is defined as the analyst's first job or the first three years of the analyst's first job as junior analyst, whichever is shorter, provided that the analyst remains at the firm for at least six months; this definition ensures that the analyst has spent a reasonable amount of time in his or her first job. *Mentors* are also required to enter into the profession, as measured by their first appearances in IBES, at least one year earlier than their protégés. To avoid extreme values in optimism metrics, an analyst is required to have at least three years of experience and cover at least three firms in a given year. All variable definitions are summarized in Appendix 2. In Panel B, superscripts of A, B, and C represent significance levels at the 1%, 5%, and 10% levels, respectively. The sample period is from 1983 to 2010.

Panel A: Summary Statistics

Main variables	Mean	Stdev	Min	P25	P50	P75	Max	Obs
Strong buy								
- Protégés	0.24	0.26	0.00	0.00	0.17	0.41	1.00	14,492
- Mentors	0.26	0.19	0.00	0.11	0.25	0.39	1.00	11,180
Upgrade Jumps								
- Protégés	0.13	0.22	0.00	0.00	0.00	0.19	1.00	13,856
- Mentors	0.12	0.15	0.00	0.01	0.06	0.16	1.00	10,630
Earnings forecast optimism								
- Protégés	50.57	27.73	1	27	51	74	100	17,454
- Mentors	51.12	18.74	1.00	40.00	50.40	62.25	100.00	17,454
Price growth optimism								
- Protégés	51.47	27.51	1	28	51	75	100	9,568
- Mentors	51.37	19.23	1.00	37.75	51.18	65.00	100.00	6,311
Other variables								
Accuracy	51.93	27.82	1	29	53	76	100	17,454
All-star analyst	0.15	0.36	0	0	0	0	1	17,454
All-star mentors	0.53	0.50	0	0	1	1	1	17,454
Brokerage size	54.83	43.92	1	20	43	76	203	17,454
Days to year-end								
- Earnings forecasts	118.32	61.70	30.00	74.78	97.67	142.42	361.00	17,454
- Price targets	192.78	49.90	0.00	168.25	189.48	212.17	365.00	10,008
- Recommendations	190.27	56.00	3.00	157.64	188.63	220.00	364.00	14,486
Firm coverage	14.82	6.42	1.00	10.22	14.06	18.47	56.00	17,454
Firm-specific experience	4.19	2.21	1.00	2.71	3.69	5.00	26.33	17,454
General experience	8.02	4.03	3	5	7	10	28	17,454
Prop. of all-star mentors	0.16	0.29	0	0	0	0.25	1	26,473
Number of companies	12.71	7.99	1	8	12	16	112	17,454
Number of industries	5.34	3.72	1	3	4	7	36	17,454
Underwriting	0.06	0.12	0.00	0.00	0.00	0.08	1.00	17,454

Table 3.1
Summary Statistics – *Continued*

Panel B: Correlations of Optimism Metrics

	Optimism metrics		Protégés StrongB	Protégés UJump	Protégés FOpt	Protégés PGOpt	Mentors StrongB	Mentors UJump	Mentors FOpt
Protégés	Strong buys	(StrongB)	1.000						
	Upgrade jumps	(UJump)	0.557^{A}	1.000					
	Earn. forecast opt.	(FOpt)	-0.001	0.005	1.000				
	Price growth opt.	(PGOpt)	0.109^{A}	0.003	0.014	1.000			
Mentors	Strong buys	(StrongB)	**0.208**^{A}	0.153^{A}	0.013	0.097^{A}	1.000		
	Upgrade jumps	(UJump)	0.182^{A}	**0.185**^{A}	-0.010	0.033^{A}	0.469^{A}	1.000	
	Earn. forecast opt.	(FOpt)	0.012	-0.001	**0.060**^{A}	0.023^{B}	0.018^{C}	0.027^{A}	1.000
	Price growth opt.	(PGOpt)	0.058^{A}	0.018	-0.010	**0.220**^{A}	0.136^{A}	-0.021^{C}	-0.021^{C}

Panel C: Distribution of Analysts and Mentors

Year	Number of Analysts	Fraction of Analysts (%)	Median Number of Mentors	Mean Number of Mentors
1985	101	6.78	3	3.73
1986	216	14.15	3	3.92
1987	272	17.87	2	3.00
1988	330	22.24	2	3.19
1989	416	26.02	3	3.32
1990	389	24.30	2	3.09
1991	468	31.56	3	3.90
1992	687	46.08	3	4.15
1993	698	41.55	3	4.63
1994	666	36.20	3	4.73
1995	705	35.80	3	6.51
1996	818	38.12	4	6.91
1997	922	38.79	4	7.81
1998	1,015	39.76	4	8.62
1999	1,156	43.46	5	9.61
2000	1,203	45.43	5	8.86
2001	1,189	47.66	6	9.67
2002	1,133	45.78	6	9.64
2003	1,082	43.70	6	9.89
2004	1,200	45.96	5	8.94
2005	1,299	48.71	4	8.06
2006	1,424	52.30	4	7.20
2007	1,441	52.88	4	7.12
2008	1,416	54.03	4	7.39
2009	1,350	54.26	3	5.62
2010	596	68.11	3	5.11
Average	853.54	39.29	3.73	6.33

Table 3.2
Spillover of Recommendation Optimism

This table reports the pooled panel regression results for recommendation optimism. The dependent variables are *Strong Buys* (Columns 1-4) and *Upgrade Jumps* (Columns 5-8), respectively. *Strong Buys* is defined as the number of strong buy recommendations initiated, upgraded, or reinitiated divided by the total number of outstanding recommendations made by an analyst in a given year. *Upgrade Jumps* is defined as the number of large upgrades to the total number of recommendation revisions made by an analyst in a given year. A revision is regarded as large if the revision in recommendation rating for a given firm is not to its immediately adjacent rating category. For instance, an upgrade is a jump when the rating is upgraded from "Sell" to "Buy." The main explanatory variables, *Mentor Strong Buys* and *Mentor Upgrade Jumps*, are constructed in the same manner. *All-Star Analyst* is a dummy indicating whether an analyst is ranked as all-star analyst in the previous year's October issue of the *Institutional Investor* magazine. *Number of Companies* and *Number of Industries* respectively refer to the number of companies and IBES industries followed by an analyst in the previous twelve months. *General Experience* is the number of years after an analyst first appears in IBES. *Firm-Specific Experience* is the number of years an analyst covers the firm. *Brokerage Size* is the number of analysts employed by a brokerage house in the previous twelve months. *Underwriting* is the proportion of firms affiliated with an analyst's brokerage firm. *Days to Year-End* refers to the average number of days of recommendations to the forthcoming fiscal year-end. *Firm Coverage* is the average coverage of an analyst's portfolio of firms. *Accuracy* is the percentile-rank accuracy following Hong and Kubik (2003). Additional details on all variables are summarized in Appendix 2. *First Brokerage House Fixed Effects* is first brokerage house fixed effects, which assign a value of one to each house that analysts and mentors both belonged to during their early careers. *Industry Fixed Effects* is the industry fixed effects based on IBES sectors. *Time Fixed Effects* is the year fixed effects. Each observation is at analyst-year level. The sample period is from 1983 to 2010. For ease of interpretation, all variables except dummy have been standardized with zero mean and unit standard deviation. All estimated coefficients have been multiplied by 100 to improve readability. Standard errors are clustered at analyst level and *t*-statistics are reported in parentheses.

Table 3.2
Spillover of Recommendation Optimism – *Continued*

	Dependent Variables:							
	Strong Buys				Upgrade Jumps			
Independent variables	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Mentor strong buys	6.276	4.741	4.437	4.341				
	(3.47)	(2.58)	(2.48)	(2.36)				
Mentor upgrade jumps					5.935	6.678	6.695	6.983
					(3.12)	(3.50)	(3.58)	(3.54)
All-star analyst		-12.066	-2.743	-0.644		-7.289	-1.566	-0.199
		(-3.34)	(-0.78)	(-0.18)		(-1.59)	(-0.34)	(-0.04)
Firm-specific experience		-3.861	-3.219	-2.626		0.684	0.811	-0.194
		(-2.20)	(-1.89)	(-1.40)		(0.35)	(0.41)	(-0.09)
General experience		1.728	0.599	-0.608		-0.886	-1.390	-1.015
		(0.78)	(0.28)	(-0.26)		(-0.39)	(-0.60)	(-0.39)
Number of companies		-3.339	-1.982	-2.331		2.433	3.613	2.763
		(-2.40)	(-1.46)	(-1.60)		(1.60)	(2.40)	(1.68)
Number of industries		0.550	-0.075	-1.003		-2.704	-3.088	-2.775
		(0.36)	(-0.05)	(-0.64)		(-0.93)	(-1.25)	(-1.21)
Brokerage size			-15.109	-14.622			-8.822	-9.728
			(-9.30)	(-8.30)			(-4.74)	(-4.94)
Underwriting			-0.970	-1.126			-4.825	-5.425
			(-0.67)	(-0.74)			(-3.57)	(-3.88)
Accuracy				0.794				2.026
				(0.59)				(1.34)
Days to year-end				-0.327				1.287
				(-0.29)				(1.15)
Firm coverage				-3.340				2.362
				(-1.91)				(1.25)
Constant	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
First brokerage house FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry FE	No	Yes	Yes	Yes	No	Yes	Yes	Yes
Time FE	No	Yes	Yes	Yes	No	Yes	Yes	Yes
Number of observations	11,702	11,702	11,702	10,400	10,773	10,773	10,773	9,639
Adjusted-R^2 (%)	15.456	20.064	21.420	21.897	14.885	18.304	18.880	20.115

Table 3.3
Spillover of Earnings Forecast and Price Target Optimism

This table reports the panel regression results for optimism in earnings forecasts and price targets. The dependent variables are respectively *Earnings Forecast Optimism* (Columns 1-4), defined as the percentile-rank optimism measure based on one-year-ahead earnings forecasts following Hong, Kubik, and Solomon (2000) and Hong and Kubik (2003), and *Price Growth Optimism* (Columns 5-8), computed based on the one-year-ahead split-adjusted price target to the split-adjusted stock price on announcement date. The main explanatory variables, *Mentor Earnings Forecast Optimism* and *Mentor Price Growth Optimism*, are constructed in the same manner. Additional details on all variables are summarized in Appendix 2. The sample period is from 1983 to 2010. For ease of interpretation, all variables except dummy have been standardized with zero mean and unit standard deviation. All estimated coefficients have been multiplied by 100 to improve readability. Standard errors are clustered at analyst level and *t*-statistics are reported in parentheses.

	Dependent Variables:							
	Earnings Forecast Optimism				Price Growth Optimism			
Independent variables	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Mentor earnings	3.955	4.039	4.175	4.092				
forecast optimism	(2.58)	(2.68)	(2.77)	(2.76)				
Mentor price growth					11.890	7.998	7.982	9.043
optimism					(5.43)	(3.55)	(3.61)	(4.07)
All-star analyst		-4.294	-0.972	-2.732		-12.356	-7.974	-5.065
		(-1.17)	(-0.26)	(-0.75)		(-2.04)	(-1.31)	(-0.83)
Firm-specific experience		-1.156	-1.093	-2.875		-5.325	-4.582	-5.356
		(-0.68)	(-0.64)	(-1.71)		(-2.50)	(-2.18)	(-2.35)
General experience		14.535	14.240	13.314		1.503	0.694	1.135
		(8.10)	(7.85)	(7.49)		(0.51)	(0.24)	(0.36)
Number of companies		1.271	1.570	1.386		0.915	1.303	0.239
		(0.88)	(1.09)	(0.93)		(0.38)	(0.55)	(0.09)
Number of industries		-3.181	-3.354	-2.912		2.689	2.263	-0.016
		(-2.06)	(-2.20)	(-1.86)		(1.14)	(0.97)	(-0.01)
Brokerage size			-5.642	-6.454			-8.384	-7.643
			(-3.67)	(-4.26)			(-3.31)	(-2.90)
Underwriting			-1.833	-1.744			5.621	4.693
			(-1.50)	(-1.43)			(2.91)	(2.33)
Accuracy				15.095				0.789
				(10.05)				(0.46)
Days to year-end				8.475				-0.848
				(7.88)				(-0.53)
Firm coverage				1.090				-12.014
				(0.68)				(-5.23)
Constant	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
First brokerage house FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry FE	No	Yes	Yes	Yes	No	Yes	Yes	Yes
Time FE	No	Yes	Yes	Yes	No	Yes	Yes	Yes
Number of observations	15,981	15,981	15,981	15,981	6,401	6,401	6,401	5,566
Adjusted-R^2 (%)	4.601	6.556	6.761	8.330	14.878	17.340	17.830	19.531

Table 3.4
Impact of Optimistic vs. Pessimistic Mentors

This table reports the pooled logistic regression results. In Panel A, the dependent variables are *Strong Buy Dummy*, *Upgrade Jump Dummy*, *Optimistic Forecast Dummy*, and *Optimistic Price Growth Dummy*, respectively. Each dummy takes one when an analyst's corresponding optimism metric in the top quintile in a given year. *Optimistic Mentors* is an indicator that takes one when a specific mentor metric is the top 10% or 20% in a given year. In Panel B, the dependent variables are *Fewer Strong Buy Dummy*, *Fewer Upgrade Jump Dummy*, *Pessimistic Forecast Dummy*, and *Pessimistic Price Growth Dummy*, respectively. Each dummy takes one when an analyst's corresponding optimism metric is in the bottom quintile in a given year. *Optimistic Mentors* is similarly defined as an indicator that takes one when a specific mentor metric is in the bottom 10% or 20% in a given year. All variables are defined in the same manner as in Table 3.2 and 3.3 with additional details summarized in Appendix 2. The sample period is from 1983 to 2010. All marginal probabilities have been multiplied by 100 to improve readability. Standard errors are clustered at analyst level and *z*-statistics are reported in parentheses.

Panel A: Baseline Regressions

	Dependent Variables:							
	Strong Buy Dummy		Upgrade Jump Dummy		Optimistic Forecast Dummy		Optimistic Price Growth Dummy	
Independent variables	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Optimistic mentors	12.823	8.097	16.871	6.579	4.841	4.303	13.470	5.283
- Top 10% indicators	(8.75)	(6.73)	(9.69)	(7.84)	(3.25)	(3.09)	(5.66)	(2.93)
Constant	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Accuracy effects	No	Yes	No	Yes	No	Yes	No	Yes
Brokerage house effects	No	Yes	No	Yes	No	Yes	No	Yes
Days to year-end effects	No	Yes	No	Yes	No	Yes	No	Yes
Firm-specific exp. effects	No	Yes	No	Yes	No	Yes	No	Yes
Firm coverage effects	No	Yes	No	Yes	No	Yes	No	Yes
General experience effects	No	Yes	No	Yes	No	Yes	No	Yes
Industry effects	No	Yes	No	Yes	No	Yes	No	Yes
Num of companies effects	No	Yes	No	Yes	No	Yes	No	Yes
Num of industries effects	No	Yes	No	Yes	No	Yes	No	Yes
Underwriting effects	No	Yes	No	Yes	No	Yes	No	Yes
Year effects	No	Yes	No	Yes	No	Yes	No	Yes
Number of observations	11,702	10,362	11,612	10,272	15,981	15,955	5,588	4,967
Pseudo-R^2 (%)	1.163	11.132	1.690	18.159	0.179	5.722	1.418	11.374
Alternative definition								
Optimistic mentors	11.407	7.731	15.229	6.131	2.796	2.021	9.228	3.084
- Top 20% indicators	(10.29)	(8.44)	(11.34)	(8.98)	(2.42)	(1.85)	(5.17)	(2.21)

Table 3.4
Impact of Optimistic vs. Pessimistic Mentors – *Continued*

Panel B: Impact of Pessimistic Mentors

	Dependent Variables:							
	Fewer Strong Buy Dummy		Fewer Upgrade Jump Dummy		Pessimistic Forecast Dummy		Pessimistic Price Growth Dummy	
Independent variables	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Pessimistic mentors	1.285	1.747	8.899	4.291	4.540	3.415	10.557	5.130
- Bottom 10% indicators	(0.68)	(0.88)	(9.24)	(7.38)	(2.83)	(2.25)	(4.43)	(2.23)
Constant/Controls/FE	Identical to Panel A							
Number of observations	11,702	10,390	11,612	9,934	15,981	15,930	5,588	5,020
Pseudo-R^2 (%)	0.007	17.484	1.357	9.983	0.137	5.056	0.837	10.304
Alternative definition								
Pessimistic mentors	8.879	8.918	6.700	2.982	4.033	3.562	9.975	4.415
- Top 20% indicators	(6.39)	(6.13)	(8.43)	(6.44)	(3.31)	(3.11)	(5.39)	(2.38)

Panel C: Impact of Optimistic and Pessimistic Mentors

Independent variables	Strong Buy Dummy		Upgrade Jump Dummy		Optimistic Forecast Dummy		Optimistic Price Growth Dummy	
Top 10% indicators								
- Optimistic mentors	12.780	8.023	15.754	6.525	4.649	3.985	12.926	4.957
	(8.66)	(6.60)	(9.00)	(7.78)	(3.10)	(2.84)	(5.41)	(2.74)
- Pessimistic mentors	-0.450	-0.843	-9.104	-0.650	-1.861	-2.715	-4.946	-3.186
	(-0.24)	(-0.51)	(-3.76)	(-0.58)	(-1.18)	(-1.82)	(-1.77)	(-1.49)
Constant/Controls/FE	Identical to Panel A							
Number of observations	11,702	10,362	11,612	10,272	15,981	15,955	5,588	4,967
Pseudo-R^2 (%)	1.164	11.137	2.089	18.167	0.201	5.771	1.549	11.024

Table 3.5
Skills Spillover

Panels A and B report the pooled panel regression results for analyst accuracy. The dependent variable is percentile-rank accuracy following Hong, Kubik, and Solomon (2000) and Hong and Kubik (2003). In Panel B, *All-Star Mentor* is a dummy indicating whether at least one of the analyst's mentors has ever been ranked as an all-star analyst in the *Institutional Investor* magazine since 1983. Panel C reports the marginal probabilities from regressing all-star analyst dummy on a host of explanatory variables and *Proportion of All-Star Mentors*, which is defined as the proportion of all-star mentors ever ranked as all-star analysts in prior *Institutional Investor* magazines. The sample period is from 1983 to 2010. All marginal probabilities have been multiplied by 100 to improve readability. Standard errors are clustered at analyst level and *z*-statistics are reported in parentheses.

Panel A: Baseline Regressions				
	Dependent Variable: Accuracy			
Independent variables	(1)	(2)	(3)	(4)
Mentor accuracy	1.300	1.071	0.971	0.328
	(0.89)	(0.78)	(0.70)	(0.26)
Constant	Yes	Yes	Yes	Yes
Control variables	Identical to Columns (1)-(4) in Table 3.2			
First brokerage house FE	Yes	Yes	Yes	Yes
Industry FE	No	Yes	Yes	Yes
Time FE	No	Yes	Yes	Yes
Num of observations	15,981	15,981	15,981	15,981
Adjusted-R^2 (%)	7.355	11.068	11.179	22.246
Panel B: All-Star Mentors				
All-star mentors × Mentor accuracy	3.657	2.940	7.492	4.264
	(1.87)	(1.67)	(2.67)	(1.71)
All-star mentors			3.547	3.736
			(0.96)	(1.11)
Mentor accuracy	0.235	-0.520	-1.696	-1.405
	(0.15)	(-0.38)	(-0.95)	(-0.88)
Constant/Control/FEs	Identical to Column (1) in Panel A	Identical to Column (4) in Panel A	Identical to Column (1) in Panel A	Identical to Column (4) in Panel A
Num of observations	15,981	15,981	15,981	15,981
Adjusted-R^2 (%)	7.412	22.280	7.470	22.288

Table 3.5

Skills Spillover – *Continued*

Panel C: Impact of Having All-Star Mentors in Early Careers				
	Dependent Variable: All-Star Analyst Dummy			
	All Years	All Years	1983-2000	2001-2010
Independent variables	(1)	(2)	(3)	(4)
Prop. of all-star mentors	5.933	5.743	7.763	4.296
	(7.50)	(7.30)	(5.83)	(5.42)
Analyst accuracy	0.153	0.136	0.257	0.054
	(7.20)	(6.29)	(6.05)	(3.18)
Analyst optimism	-0.013	-0.010	-0.003	0.003
	(-0.67)	(-0.49)	(-0.08)	(0.17)
Brokerage size	8.942	8.702	12.645	5.242
	(23.85)	(23.17)	(19.53)	(12.87)
General experience	0.263	-0.142	1.749	-0.659
	(0.44)	(-0.24)	(1.43)	(-1.29)
Firm-specific experience	7.866	7.708	11.242	4.425
	(11.19)	(10.79)	(9.32)	(5.97)
Number of companies		2.251	4.051	0.971
		(5.58)	(5.49)	(2.75)
Number of industries		-0.188	-1.306	0.522
		(-0.57)	(-2.18)	(1.82)
Constant	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes
Industry fixed effects	Yes	Yes	Yes	Yes
Number of observations	26,473	26,473	13,976	12,497
Pseudo-R^2 (%)	32.429	32.942	30.905	35.231
Constant/Control/FEs	Identical to Panel A			

Table 3.6
Robustness Checks

This table reports the results of different robustness checks. *Active analysts* focuses on active analysts who cover at least eight firms per year. *After 1993* excludes forecasts made before the year 1993. As stock recommendation and price target data are not available until 1993 and 1999, respectively, the estimates reported below under Columns (1) and (2) in the row *After 1993* are identical to those reported in prior tables; however, they are re-iterated for illustration purposes. *After Regulation FD* uses sample data on or after year 2001, the year after the Regulation Fair Disclosure was enacted. *Current Brokerage House Fixed Effects* assigns a value of one to each house where an analyst is currently employed. *Year of First Entry Fixed Effects,* which is a series of dummies each taking a value of one for the year when analysts first appear in the IBES dataset, controls for analysts' year of entry. *Year-Sector Fixed Effects* includes year-sector fixed effects in regressions. Standard errors are clustered at analyst level and *t*-statistics are reported in parentheses. *Fama-MacBeth (FM) Regressions* report the regression results estimated using annual time-series cross-sectional Fama-MacBeth (1973) regressions. Except for year and first brokerage house fixed effects, which are not included, the regressions for the annual FM regressions are identical to those in Tables 3.2 and 3.3. *Positive (Negative)* refers to the number of positive (negative) estimates. *Significant* refers to the significance at the 5% level. Standard errors are adjusted using Newey-West (1987) with a 4-year lag and reported in brackets. All variables except dummy have been standardized with zero mean and unit standard deviation. All estimated coefficients have been multiplied by 100.

Table 3.6
Robustness Checks – *Continued*

	Dependent Variables:			
	Strong Buys	Upgrade Jumps	Earnings Forecast Optimism	Price Growth Optimism
Descriptions	(1)	(2)	(3)	(4)
Active analysts	4.059	5.296	3.628	10.235
	(2.12)	(2.49)	(2.28)	(4.04)
After 1993	4.341	6.983	4.339	9.043
	(2.36)	(3.54)	(2.88)	(4.07)
After Regulation FD	4.531	7.812	3.508	9.298
	(2.13)	(3.00)	(1.92)	(4.05)
Current brokerage house FEs	2.535	4.046	4.341	9.663
	(2.56)	(3.62)	(3.23)	(4.46)
Year of entry fixed effects	5.339	7.204	3.789	8.973
	(2.78)	(3.68)	(2.58)	(4.07)
Year-sector fixed effects	3.998	6.871	4.382	8.616
	(2.14)	(3.51)	(2.94)	(3.61)
Fama-MacBeth regressions	14.766	13.962	5.853	10.007
	[5.84]	[3.31]	[3.96]	[4.88]
#Positive	17	16	24	11
#Positive+significant	14	14	6	8
#Negative	0	1	2	0
#Negative+significant	0	0	0	0
Constant/Control/FEs	Identical to Column (4) in Table 3.2	Identical to Column (8) in Table 3.2	Identical to Column (4) in Table 3.3	Identical to Column (8) in Table 3.3

Table 3.7
Small Brokerage Houses: A Hybrid Estimation

This table reports the pooled panel regression results based on hybrid mentor optimism metrics. For those small brokerage houses where analysts do not have mentors who cover the same sectors in their early careers, their missing mentor optimism metrics are replaced with the optimism metrics generated based on all other seniors regardless of whether they cover the same sectors. Panel A reports the baseline results estimated under this hybrid estimation for brokerage houses with fewer than five employees. Panel B uses alternative classifications to define "small brokerage house." In Panel B, the number in brackets refers to the number of observations. The sample period is from 1983 to 2010. For ease of interpretation, all variables except dummy have been standardized with zero mean and unit standard deviation. All estimated coefficients have been multiplied by 100 to improve readability. Standard errors are clustered at analyst level and *t*-statistics are reported in parentheses.

Panel A: Baseline Regressions

	Dependent Variables:			
	Strong Buys	Upgrade Jumps	Earnings Forecast Optimism	Price Growth Optimism
Independent variables	(1)	(2)	(3)	(4)
Hybrid mentor optimism	4.725	6.902	3.310	9.192
	(2.56)	(3.54)	(2.29)	(4.10)
Constant	Yes	Yes	Yes	Yes
First brokerage house FE	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes
Control variables	Identical to Column (4) in Table 3.2	Identical to Column (8) in Table 3.2	Identical to Column (4) in Table 3.3	Identical to Column (8) in Table 3.3
Number of observations	10,705	9,908	16,863	5,729
Adjusted-R^2 (%)	21.777	20.066	8.554	19.609

Panel B: Alternative Classifications of Small Brokerage Houses

Num of analysts in the first brokerage house				
- Fewer than 10 analysts	4.482	6.524	2.803	7.885
	(2.40)	(3.35)	(1.99)	(3.56)
	[Obs=11,136]	[10,277]	[17,706]	[5,984]
- Fewer than 15 analysts	4.914	6.348	2.522	7.359
	(2.68)	(3.31)	(1.84)	(3.37)
	[Obs=11,447]	[10,567]	[18,677]	[6,191]
- Fewer than 20 analysts	5.229	6.532	2.138	7.247
	(2.87)	(3.41)	(1.59)	(3.32)
	[Obs=11,678]	[10,782]	[19,275]	[6,369]
Constant/Controls/FEs	Identical to Panel A			

Table 4.1
Smart Market: Market Reaction Tests

This table reports the pooled panel regression results for market reaction. The dependent variable refers to the size-adjusted cumulative return to the revision made by analyst *i* for firm *j* at time *t* in the windows of [-1,+1], [-1,+3], [-1,+5], or [-1,+10] around the revision date. Day 0 refers to the forecast revision date (or the preceding trading day if Day 0 is a non-trading day). [-*j*,+*k*] refers to the period from trading day *j* days before the forecast revision date to *k* days after the revision date. Size-adjusted cumulative returns are defined as the buy-and-hold return of firm *j* minus the buy-and-hold return for an equal-weighted portfolio of firms in the same NYSE size decile formed at the beginning of each year. In Panel A, *Optimistic Mentor Dummy*, defined as a dummy that takes one when *Mentor Earnings Forecast Optimism* is in the top decile and zero otherwise, is interacted with *Revision Signal*, which takes the value of +1 (-1) when an analyst's revised forecast is above (below) both his or her own prior forecast estimate and the prior consensus, and zero otherwise (Gleason and Lee (2003)). *Revision Magnitude* is defined as the difference between an analyst's current and previous earnings forecast for the same firm in the same fiscal-year, divided by the share price two days prior to the announcements of the revision. In Panel B, *Pessimistic Mentor Dummy* is defined in a similar fashion. Additional details on all variables are summarized in Appendix 2. To conserve space, the estimates of various control variables are reported in Online Appendix 2 In Panel C, all observations are sorted into terciles (i.e., high, mid, and low) based on the level of institutional ownership reported in the prior quarter's Thomson-Reuters Institutional Holdings (13f) database. The sample period is from 1983 to 2009. For ease of interpretation, all variables except dummy have been standardized with zero mean and unit standard deviation. All estimated coefficients have been multiplied by 100 to improve readability. Standard errors are clustered at month level and *t*-statistics are reported in parentheses.

Table 4.1
Smart Market: Market Reaction Tests – *Continued*

Panel A: Baseline Regressions								
	Dependent Variable: Size-Adjusted Cumulative Return							
	[-1,+1]		[-1,+3]		[-1,+5]		[-1,+10]	
Independent variables	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Optimistic mentor dummy	-0.489	-0.478	-0.463	-0.446	-0.460	-0.442	-0.287	-0.258
× Revision signal	(-4.10)	(-4.00)	(-3.83)	(-3.61)	(-3.47)	(-3.21)	(-2.10)	(-1.86)
Optimistic mentor dummy	0.099	-0.178	0.493	0.228	0.292	0.009	0.378	0.124
	(0.22)	(-0.39)	(0.96)	(0.44)	(0.56)	(0.02)	(0.71)	(0.23)
Revision signal	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Revision magnitude	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Affiliated dummy	No	Yes	No	Yes	No	Yes	No	Yes
All-star analyst	No	Yes	No	Yes	No	Yes	No	Yes
Book-to-market ratio	No	Yes	No	Yes	No	Yes	No	Yes
Brokerage size	No	Yes	No	Yes	No	Yes	No	Yes
Days since last forecast	No	Yes	No	Yes	No	Yes	No	Yes
Days to year-end	No	Yes	No	Yes	No	Yes	No	Yes
Forecast frequency	No	Yes	No	Yes	No	Yes	No	Yes
Firm size	No	Yes	No	Yes	No	Yes	No	Yes
Firm-specific experience	No	Yes	No	Yes	No	Yes	No	Yes
General experience	No	Yes	No	Yes	No	Yes	No	Yes
Institutional ownership	No	Yes	No	Yes	No	Yes	No	Yes

Table 4.1

Smart Market: Market Reaction Tests – *Continued*

Panel A: Baseline Regressions – *Continued*								
	Dependent Variable: Size-Adjusted Cumulative Return							
	[-1,+1]		[-1,+3]		[-1,+5]		[-1,+10]	
Independent variables	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel B: Optimistic and Pessimistic Mentors								
Optimistic mentor dummy × Revision signal	-0.491	-0.259	-0.465	-0.258	-0.460	-0.293	-0.286	-0.048
	(-4.10)	(-1.98)	(-3.84)	(-2.09)	(-3.46)	(-1.99)	(-2.09)	(-0.34)
Pessimistic mentor dummy × Revision signal	-0.029	-0.038	-0.049	-0.067	-0.013	-0.032	0.010	-0.011
	(-0.28)	(-0.37)	(-0.43)	(-0.58)	(-0.11)	(-0.27)	(0.08)	(-0.09)
Alternative definition								
Top 20% optimistic mentor × Revision signal	-0.370	-0.318	-0.323	-0.262	-0.300	-0.237	-0.269	-0.192
	(-3.01)	(-2.63)	(-2.68)	(-2.21)	(-2.25)	(-1.78)	(-2.03)	(-1.47)
Top 20% pessimistic mentor × Revision signal	-0.127	-0.167	-0.190	-0.234	-0.124	-0.167	-0.083	-0.129
	(-0.97)	(-1.27)	(-1.45)	(-1.79)	(-0.97)	(-1.31)	(-0.63)	(-0.99)
Constant/Control/FEs	Identical to Panel A							
Panel C: Smart Institutional Investors								
	Estimated Coefficients on *Optimistic Mentor Dummy* × *Revision Signal*							
Level of institutional ownership	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
- High inst. ownership	-0.888	-0.874	-0.858	-0.840	-0.769	-0.748	-0.589	-0.566
	(-4.20)	(-4.17)	(-3.82)	(-3.77)	(-3.35)	(-3.24)	(-2.72)	(-2.67)
- Mid inst. ownership	-0.279	-0.277	-0.191	-0.187	-0.232	-0.228	-0.153	-0.149
	(-1.61)	(-1.59)	(-1.07)	(-1.04)	(-1.20)	(-1.17)	(-0.80)	(-0.78)
- Low inst. ownership	-0.158	-0.145	-0.208	-0.189	-0.261	-0.240	-0.033	-0.004
	(-0.76)	(-0.70)	(-0.97)	(-0.89)	(-1.19)	(-1.11)	(-0.14)	(-0.02)
Constant/Control/FEs	Identical to Panel A							

Table 4.2
Junior Optimism and Span of First Job

This table reports the pooled panel regression results. The dependent variables, *Strong Buys*, *Upgrade Jumps*, *Earnings Forecast Optimism*, and *Price Growth Optimism*, are identical to those reported in Tables 3.2 and 3.3. In Panel A, *Juniors* are those colleagues who work in the same brokerage house (i.e., same house), who cover the same sector (i.e., same team), but have fewer years of professional experience (i.e., rookies) during analysts' early careers. The first subpanel of Panel A reports the baseline regression results where analysts who join the same house in the same year are counted as *Juniors*, whereas the second subpanel excludes analysts who join the same house in the same year. Panel B sorts analysts based on the span of their first jobs. For ease of interpretation, all variables except dummy have been standardized with zero mean and unit standard deviation. All estimated coefficients have been multiplied by 100 to improve readability. Standard errors are clustered at analyst level and *t*-statistics are reported in parentheses.

Table 4.2

Junior Optimism and Span of First Job – *Continued*

Panel A: Junior Optimism				
	Baseline Results			
Junior optimism	2.826	2.547	-1.471	0.036
	(1.23)	(1.03)	(-0.71)	(0.01)
Mentor optimism	3.005	5.808	6.174	12.677
	(1.14)	(1.62)	(2.09)	(4.12)
Constant/Control/FEs	Identical to Column (4) in Table 3.2	Identical to Column (8) in Table 3.2	Identical to Column (4) in Table 3.3	Identical to Column (8) in Table 3.3
	Alternative *Juniors* Definition			
Junior optimism	2.992	4.260	-2.379	-2.055
	(1.08)	(1.52)	(-0.88)	(-0.65)
Mentor optimism	5.804	6.742	1.800	14.122
	(1.91)	(1.44)	(0.44)	(3.72)
Constant/Control/FEs	Identical to Column (4) in Table 3.2	Identical to Column (8) in Table 3.2	Identical to Column (4) in Table 3.3	Identical to Column (8) in Table 3.3
Panel B: Span of First Job				
Two years or more	4.712	6.805	4.008	9.083
	(1.85)	(2.53)	(2.09)	(3.01)
Less than one year	1.235	12.495	-3.587	6.776
	(0.26)	(4.06)	(-0.77)	(1.11)

Table 5.1
Summary Statistics

Panel A provides the summary statistics for the main variables in the sample. Panel B reports the pairwise Pearson correlations. The main samples are obtained from IBES, whereas price information is obtained from CRSP. *Recession* (*Non-Recession*) refers to recession (non-recession) analysts. *Analyst accuracy* (*optimism*) is the percentile-rank accuracy (optimism) following Hong, Kubik, and Solomon (2000) and Hong and Kubik (2003). *Recession* is an indicator that takes one when an analyst first appears in the IBES dataset as a lead analyst during the years when the annual employment growth in the analyst labor market is negative (i.e., during the years of 1988-91, 2002-03, or 2009-10), or zero otherwise. *Boom* as a dummy that takes one when an analyst first appears in IBES during the years when the non-negative annual employment growth in the analyst labor market is in its top tertile, or zero otherwise (i.e., during the years of 1984, 1986-1987, 1993-1994, 1998, or 2000), or zero otherwise. To avoid extreme values in optimism and accuracy metrics, an analyst is required to have at least three forecasts in a given year. All variable definitions are summarized in Appendix 3. *t*-statistics are clustered at analyst level.

Main variables	Recess.	Non-Rec.	Diff.	*t*-stat	Mean	Std.	P25	P50	P75	Num.
Analyst optimism	48.806	49.497	-0.690	-3.64	49.343	11.135	43	49	56	56,906
Analyst accuracy	49.972	51.087	-1.115	-5.80	50.839	11.320	45	52	58	56,906
All-star analyst	0.091	0.100	-0.009	-1.17	0.098	0.298	0	0	0	56,906
Ever-star analyst	0.152	0.174	-0.022	-1.99	0.170	0.375	0	0	0	56,906
Brokerage size										
- All jobs	43.280	45.860	-2.580	-2.62	45.287	40.562	15	33	60	56,906
- First jobs	44.025	39.061	4.963	3.94	40.164	47.699	9	24	55	56,906
Days to year-end	116.659	119.007	-2.347	-3.42	118.485	56.404	77	102	144	56,906
Firm-specific exp.	3.161	3.111	0.051	0.93	3.122	2.237	2	3	4	56,906
Firm coverage	14.483	15.396	-0.912	-5.55	15.193	7.262	10	14	19	56,906
General experi.	5.507	5.681	-0.174	-1.63	5.642	4.737	2	4	8	56,906
Num of companies	12.419	11.816	0.604	2.85	11.950	8.572	6	10	15	56,906
Num of industries	5.675	5.528	0.148	1.29	5.561	4.139	3	5	7	56,906
Underwriting	0.049	0.046	0.003	1.29	0.047	0.107	0	0	0	56,906
Recession					0.222	0.416	0	0	0	56,906
Boom					0.262	0.440	0	0	1	56,906

Table 5.2
Recession Analysts and Forecast Optimism

This table reports the pooled panel regression results for forecast optimism. *Forecast Optimism* is defined as the percentile-rank optimism measure based on one-year-ahead earnings forecasts following Hong, Kubik, and Solomon (2000) and Hong and Kubik (2003). *Recession* is an indicator that takes one when an analyst first appears in the IBES dataset as a lead analyst during the years when the annual employment growth in the analyst labor market is negative. *All-Star Analyst* is a dummy indicating whether an analyst is ranked as all-star analyst in the previous year's October issue of the *Institutional Investor* magazine. *Brokerage Size* is the number of analysts employed by a brokerage house in the previous twelve months. *Days to Year-End* refers to the average number of days of recommendations to the forthcoming fiscal year-end. *Firm Coverage* is the average coverage of an analyst's portfolio of firms. *Accuracy* is the percentile-rank accuracy following Hong and Kubik (2003). *Firm-Specific Experience* is the number of years an analyst covers the firm. *General Experience* is the number of years after an analyst first appears in IBES. *Number of Companies* and *Number of Industries* respectively refer to the number of companies and IBES industries followed by an analyst in the previous twelve months. *Underwriting* is the proportion of firms affiliated with an analyst's brokerage firm. Additional details on all variables are summarized in Appendix 3. *Brokerage House Fixed Effects* assign a value of one to each brokerage house that an analyst belongs to. *Industry Fixed Effects* is the industry fixed effects based on IBES sectors. *Time Fixed Effects* is the year fixed effects. Each observation is at analyst-year level. The sample period is from 1983 to 2010. For ease of interpretation, all variables except dummy have been standardized with zero mean and unit standard deviation. All estimated coefficients have been multiplied by 100 to improve readability. Standard errors are clustered at analyst-and-year or analyst level and *t*-statistics are reported in parentheses.

Table 5.2 – *Continued*
Recession Analysts and Forecast Optimism

	Dependent Variables: *Forecast Optimism*				
Independent variables	(1)	(2)	(3)	(4)	(5)
Recession	-4.335	-4.139	-3.147	-3.818	-3.940
	(-3.64)	(-3.51)	(-2.61)	(-3.16)	(-3.27)
All-star analyst		-4.046	-4.640	-2.741	-2.108
		(-2.88)	(-3.27)	(-1.87)	(-1.46)
Firm-specific experience		-0.213	-0.208	0.096	-0.587
		(-0.30)	(-0.29)	(0.14)	(-0.81)
General experience		8.154	9.371	9.681	9.707
		(11.35)	(12.38)	(12.96)	(12.97)
Number of companies			-0.389	0.322	0.300
			(-0.82)	(0.70)	(0.60)
Number of industries			0.183	-1.152	-0.533
			(0.36)	(-2.45)	(-1.01)
Analyst accuracy				0.023	2.308
				(0.03)	(2.67)
Brokerage size				0.278	0.385
				(0.27)	(0.37)
Days to year-end					5.544
					(12.46)
Firm coverage					0.744
					(1.12)
Underwriting					-0.484
					(-0.91)
Constant	Yes	Yes	Yes	Yes	Yes
Industry FEs	No	Yes	Yes	No	Yes
Time FEs	No	No	Yes	Yes	Yes
Brokerage house FEs	No	No	No	Yes	Yes
Num of observations	56,906	56,906	56,906	56,906	56,906
Adjusted-R^2 (%)	0.065	1.372	1.756	5.409	5.811

Table 5.3
Recession Analysts and Forecast Accuracy

This table reports the pooled OLS regression results. The dependent variable, *Forecast Accuracy*, is percentile-rank accuracy following Hong, Kubik, and Solomon (2000) and Hong and Kubik (2003). The regression specifications are identical to those in Table 5.2. Each observation is at analyst-year level. The sample period is from 1983 to 2010. For ease of interpretation, all variables except dummy have been standardized with zero mean and unit standard deviation. All estimated coefficients have been multiplied by 100 to improve readability. Standard errors are clustered at analyst level and *t*-statistics are reported in parentheses.

	Dependent Variables: *Forecast Accuracy*				
Independent variables	(1)	(2)	(3)	(4)	(5)
Recession	-7.006	-6.659	-5.458	-5.122	-4.176
	(-5.80)	(-5.88)	(-4.72)	(-4.61)	(-4.04)
All-star analyst		20.006	17.970	9.300	6.134
		(15.51)	(13.59)	(6.97)	(5.05)
Firm-specific experience		3.224	2.408	2.121	4.678
		(4.28)	(3.19)	(3.00)	(6.90)
General experience		5.380	6.501	4.791	4.772
		(7.35)	(8.50)	(6.46)	(6.84)
Number of companies			4.232	5.011	1.924
			(6.72)	(8.77)	(3.82)
Number of industries			-2.953	0.038	-0.165
			(-5.37)	(0.08)	(-0.34)
Analyst optimism				0.022	1.983
				(0.03)	(2.67)
Brokerage size				-1.753	-1.510
				(-1.72)	(-1.61)
Days to year-end					-28.944
					(-77.48)
Firm coverage					0.353
					(0.59)
Underwriting					-0.608
					(-1.22)
Constant	Yes	Yes	Yes	Yes	Yes
Industry FEs	No	Yes	Yes	No	Yes
Time FEs	No	No	Yes	Yes	Yes
Brokerage house FEs	No	No	No	Yes	Yes
Num of observations	56,906	56,906	56,906	56,906	56,906
Adjusted-R^2 (%)	0.166	2.628	3.088	11.402	21.989

Table 5.4
Robustness Checks

This table reports the results of different robustness checks. *After 1993* excludes forecasts made before the year 1993. *After Regulation FD* uses sample data on or after year 2001, the year after the Regulation Fair Disclosure was enacted. *Left Censored at 1984* excludes those analysts who first appear in the IBES before 1984. *At Least 3 Years of Experience* includes those analysts who have at least three years of experience. *Controlling for Industry-Year FEs* controls for year-industry fixed effects (instead of year and industry fixed effects) in regressions. *Price-Scaled Dependent Variables* replace the dependent variables with (absolute) forecast error variables relative to actual earnings, scaled by lag price in the prior month. *Non-linear control variables* replace *Firm-specific/General Experience*, *Number of Companies/Industries*, *Brokerage Size*, *Days to Year-End*, and *Firm Coverage* with the natural logarithm of the corresponding variables of interest. *NBER recession* replaces *Recession* with an indicator which takes one when analysts first appear during the NBER recessions. Standard errors are clustered at analyst level and *t*-statistics are reported in parentheses. *FM Regressions* report the regression results estimated using annual time-series cross-sectional Fama-MacBeth (1973) regressions. Except for year and brokerage house fixed effects, which are not included, the regressions for the annual FM regressions are identical to those in Tables 5.2 and 5.3. *Positive (Negative)* refers to the number of positive (negative) estimates. *Significant* refers to the significance at the 5% level. Standard errors are adjusted using Newey-West (1987) with a 4-year lag and reported in brackets. All variables except dummy have been standardized with zero mean and unit standard deviation. All estimated coefficients have been multiplied by 100. The details of each specification are in Appendix 4.

	Dependent Variables:									
	Forecast Optimism					*Forecast Accuracy*				
	Estimated Coefficients on *Recession*									
Descriptions	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
After 1993	-5.050	-5.444	-5.346	-4.961	-5.106	-4.363	-5.307	-5.640	-5.121	-4.117
	(-3.85)	(-4.24)	(-4.14)	(-3.86)	(-4.00)	(-3.35)	(-4.32)	(-4.53)	(-4.36)	(-3.77)
After Reg FD	-6.021	-4.503	-4.442	-4.323	-4.205	-6.540	-4.936	-4.752	-3.869	-3.883
	(-3.47)	(-2.67)	(-2.64)	(-2.63)	(-2.58)	(-3.95)	(-3.15)	(-3.06)	(-2.58)	(-2.76)
Left censored	-2.709	-3.477	-4.174	-4.712	-4.758	-5.382	-6.667	-6.395	-6.215	-5.503
at 1984	(-2.27)	(-2.92)	(-3.40)	(-3.83)	(-3.88)	(-4.42)	(-5.80)	(-5.39)	(-5.49)	(-5.23)
At least 3 years	-8.325	-7.435	-6.568	-7.768	-4.768	-9.689	-8.221	-4.958	-4.423	-4.224
of experience	(-4.06)	(-3.68)	(-3.13)	(-3.74)	(-3.75)	(-4.83)	(-4.39)	(-2.53)	(-2.42)	(-2.47)
Controlling for	-4.307	-3.012	-3.014	-3.715	3.751	-7.131	-5.450	-5.413	-5.008	-4.156
industry-year FEs	(-3.51)	(-2.49)	(-2.50)	(-3.07)	(-3.11)	(-5.70)	(-4.69)	(-4.65)	(-4.48)	(-4.01)
Price-scaled	-6.375	-6.747	-1.859	-2.114	-2.192	-7.266	-7.844	-1.954	-1.618	-2.228
dependent var.	(-7.79)	(-8.32)	(-2.50)	(-2.75)	(-2.90)	(-7.38)	(-8.40)	(-2.24)	(-1.76)	(-2.59)
Non-linear	-4.335	-4.391	-3.050	-3.609	-3.762	-7.006	-7.036	-5.717	-5.511	-4.335
control var.	(-3.64)	(-3.72)	(-2.53)	(-2.98)	(-3.12)	(-5.80)	(-6.23)	(-4.95)	(-4.98)	(-4.22)
NBER recession	2.019	0.383	-2.725	-3.012	-2.652	2.038	-2.235	-4.553	-5.694	-6.395
	(1.53)	(0.30)	(-1.99)	(-2.19)	(-1.93)	(1.48)	(-1.75)	(-3.42)	(-4.37)	(-5.28)
Controls/FEs	Identical to the corresponding columns in Table 5.2					Identical to the corresponding columns Table 5.3				

Table 5.4 – *Continued*
Robustness Checks

	Dependent Variables:									
	Forecast Optimism					*Forecast Accuracy*				
Descriptions	Estimated Coefficients on *Recession*									
FM regressions	-2.608	-4.037	-4.097	-4.194	-3.785	-4.916	-3.640	-3.342	-3.452	-3.450
	(-2.16)	(-3.15)	(-3.10)	(-3.31)	(-3.29)	(-1.75)	(-3.40)	(-3.32)	(-3.48)	(-3.47)
#Negative	15	20	19	18	18	16	17	17	18	19
#Negative+sig.	5	7	6	7	4	11	7	6	5	7
#Positive	8	3	4	5	5	7	6	6	5	4
#Positive+sig.	1	0	0	0	0	4	1	1	1	0
Controls/FEs	Identical to the corresponding columns in Table 5.2					Identical to the corresponding columns Table 5.3				

Table 5.5
Diffusion of Experience

This table reports the panel regression results for boom and recession analysts. The dependent variables are *Forecast Optimism* (columns 1-5) and *Forecast Accuracy* (columns 6-10), respectively. The baseline regressions are identical to those in Tables 5.2 and 5.3, respectively. *Young Analyst* is a dummy that takes one during the first three years after an analyst appears in IBES. Additional details on all variables are summarized in Appendix 3. The sample period is from 1983 to 2010. For ease of interpretation, all variables except dummy have been standardized with zero mean and unit standard deviation. All estimated coefficients have been multiplied by 100 to improve readability. Standard errors are clustered at analyst level and *t*-statistics are reported in parentheses.

Panel A: Young Analysts										
	Dependent Variables:									
	Forecast Optimism					*Forecast Accuracy*				
	Estimated Coefficients on *Recession*									
Descriptions	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Recession	-4.669	-4.807	-4.349	-4.930	-5.039	-4.849	-4.729	-2.895	-2.875	-2.234
	(-3.49)	(-3.60)	(-3.16)	(-3.59)	(-3.70)	(-3.73)	(-3.74)	(-2.19)	(-2.31)	(-1.94)
Young analyst	-0.590	-1.061	-3.261	-4.302	-4.013	-3.558	-1.212	-1.004	2.669	0.735
	(-0.54)	(-0.96)	(-2.75)	(-3.64)	(-3.41)	(-3.23)	(-1.09)	(-0.84)	(2.28)	(0.66)
Recession × Young analyst	1.445	1.515	3.270	3.093	3.046	-4.755	-4.766	-6.979	-6.213	-5.361
	(0.71)	(0.74)	(1.45)	(1.35)	(1.33)	(-2.40)	(-2.40)	(-3.16)	(-2.85)	(-2.62)
Controls	Identical to the corresponding columns in Table 5.2					Identical to the corresponding columns Table 5.3				
Num of obs.	56,906	56,906	56,906	56,906	56,906	56,906	56,906	56,906	56,906	56,906
Adj.-R^2 (%)	1.314	1.372	1.773	5.440	5.838	1.850	2.656	3.131	11.424	22.004
Panel B: General Experience										
Recession	-4.014	-4.112	-3.012	-3.717	-3.849	-7.011	-6.944	-6.128	-5.701	-4.664
	(-3.36)	(-3.45)	(-2.44)	(-2.98)	(-3.10)	(-6.00)	(-6.07)	(-5.16)	(-4.95)	(-4.33)
General experience	7.767	8.205	9.487	9.764	9.782	8.187	4.844	5.926	4.312	4.371
	(15.71)	(11.08)	(12.19)	(12.72)	(12.72)	(15.52)	(6.37)	(7.49)	(5.64)	(6.09)
Recession × General exprien.	-0.379	-0.309	-0.710	-0.501	-0.458	3.731	3.228	3.524	2.887	2.440
	(-0.34)	(-0.28)	(-0.62)	(-0.43)	(-0.40)	(3.46)	(3.00)	(3.17)	(2.63)	(2.38)
Controls	Identical to the corresponding columns in Table 5.2					Identical to the corresponding columns Table 5.3				
Num of obs.	56,906	56,906	56,906	56,906	56,906	56,906	56,906	56,906	56,906	56,906
Adj.-R^2 (%)	1.314	1.371	1.756	5.408	5.810	1.822	2.659	3.120	11.423	22.003

Table 5.6
Boom and Recession

This table reports the panel regression results for boom and recession analysts. The dependent variables are *Forecast Optimism* (columns 1-5) and *Forecast Accuracy* (columns 6-10), respectively. The baseline regressions in Panels A and B are identical to those in Tables 5.2 and 5.3, respectively. *Boom* is a dummy takes one when an analyst first appears in IBES during the years when the non-negative annual employment growth in analyst labor market is in its top tertile, or zero otherwise. Panel C (D) reports the estimated coefficients using regression specification 5 (10) in Panel A. The specifications of various robustness checks follow those in Table 5.4. Additional details on all variables are summarized in Appendix 3. The sample period is from 1983 to 2010. For ease of interpretation, all variables except dummy have been standardized with zero mean and unit standard deviation. All estimated coefficients have been multiplied by 100 to improve readability. Standard errors are clustered at analyst level and *t*-statistics are reported in parentheses.

Panel A: Boom										
	Dependent Variables:									
	Forecast Optimism					*Forecast Accuracy*				
	Estimated Coefficients on *Recession*									
Descriptions	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Boom	3.098	2.706	3.194	3.193	3.103	4.477	4.337	3.877	3.550	3.456
	(2.81)	(2.48)	(2.90)	(2.89)	(2.82)	(3.86)	(3.93)	(3.47)	(3.39)	(3.54)
Controls/FEs	Identical to the corresponding columns in Table 5.2					Identical to the corresponding columns Table 5.3				
Num of obs.	56,906	56,906	56,906	56,906	56,906	56,906	56,906	56,906	56,906	56,906
Adj.-R^2 (%)	0.036	1.341	1.763	5.402	5.799	0.075	2.548	3.051	11.370	21.980
Panel B: Boom and Recession										
Boom	1.995	1.630	2.518	2.297	2.156	2.635	2.604	2.508	2.260	2.473
	(1.71)	(1.41)	(2.15)	(1.96)	(1.85)	(2.15)	(2.24)	(2.12)	(2.02)	(2.37)
Recession	-3.664	-3.593	-2.254	-2.999	-3.172	-6.119	-5.788	-4.570	-4.316	-3.294
	(-2.91)	(-2.88)	(-1.76)	(-2.34)	(-2.48)	(-4.78)	(-4.84)	(-3.73)	(-3.64)	(-2.98)
Controls/FEs	Identical to Panel A									
Num of obs.	56,906	56,906	56,906	56,906	56,906	56,906	56,906	56,906	56,906	56,906
Adj.-R^2 (%)	0.077	1.380	1.775	5.424	5.824	0.188	2.649	3.106	11.416	22.006

Table 5.6 – *Continued*
Boom and Recession

	After 1993	After Reg FD	Left censored	3 years	Industry-year FEs	Price-scaled dep var.	Non-linear controls	FM
Panel C: Robustness Checks on *Forecast Optimism*								
	Estimated Coefficients:							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Boom	2.450	3.830	2.060	1.123	2.152	0.598	2.136	1.632
	(1.90)	(2.27)	(1.76)	(0.92)	(1.84)	(0.72)	(1.83)	(1.66)
Recession	-4.265	-3.187	-3.154	-4.345	-2.983	-1.979	-3.002	-3.859
	(-3.14)	(-1.86)	(-2.48)	(-3.22)	(-2.33)	(-2.45)	(-2.35)	(-2.91)
Controls/FEs	Identical to column (5) in Panel B							
Panel D: Robustness Checks on *Forecast Accuracy*								
Boom	1.083	-1.712	2.395	3.053	2.581	0.333	2.629	0.689
	(0.93)	(-1.21)	(2.29)	(1.78)	(2.47)	(0.36)	(2.54)	(0.37)
Recession	-3.745	-4.337	-3.257	-3.074	-3.236	-2.109	-3.398	-2.232
	(-3.19)	(-2.91)	(-2.95)	(-1.68)	(-2.92)	(-2.29)	(-3.09)	(-2.10)
Controls/FEs	Identical to column (10) in Panel B							

Table 5.7
Ever Star

This table reports the pooled logistic regression results for recession analysts. The dependent variable is *Ever-star dummy*, which takes one after an analyst is ranked as all-star analyst in *Institutional Investor* magazine. *Young analyst* is a dummy that takes one during the first three years after an analyst appears in IBES. The sample period is from 1983 to 2010. For ease of interpretation, all variables except dummy have been standardized with zero mean and unit standard deviation. All marginal probabilities have been multiplied by 100 to improve readability. Standard errors are clustered at analyst level and *z*-statistics are reported in parentheses.

Panel A: Recession Analysts					
	Dependent Variable: *Ever-Star* Dummy				
Independent variables	(1)	(2)	(3)	(4)	(5)
Recession	-2.278	-2.294	-2.364	-2.196	-1.500
	(-1.93)	(-2.12)	(-2.23)	(-2.23)	(-1.70)
Constant	Yes	Yes	Yes	Yes	Yes
Firm-specific exp.	No	Yes	Yes	Yes	Yes
General experience	No	Yes	Yes	Yes	Yes
Number of companies	No	No	Yes	Yes	Yes
Number of industries	No	No	Yes	Yes	Yes
Brokerage size	No	No	No	Yes	Yes
Days to year-end	No	No	No	No	Yes
Firm coverage	No	No	No	No	Yes
Underwriting	No	No	No	No	Yes
Num of obs.	56,906	56,906	56,906	56,906	56,906
Pseudo-R^2 (%)	0.067	14.924	16.067	20.721	24.319
Panel B: Boom Analysts					
Boom	-0.967	-0.434	-0.553	-1.101	-0.829
	(-0.87)	(-0.44)	(-0.57)	(-1.21)	(-1.00)
Controls/FEs	Identical to Panel A				
Panel B: Boom and Recession Analysts					
Boom	-1.787	-1.216	-1.374	-1.895	-1.376
	(-1.53)	(-1.16)	(-1.33)	(-1.98)	(-1.58)
Recession	-2.861	-2.697	-2.819	-2.817	-1.951
	(-2.30)	(-2.36)	(-2.52)	(-2.71)	(-2.09)
Controls/FEs	Identical to Panel A				

Appendix 1
Reconstructing Employment History Panel

This appendix provides further details on the construction of the panel on employment history. The source of the sample of earnings forecasts (1983-2010), price targets (1999-2010), and stock recommendations (1993-2010) is the Thomson Reuters' IBES dataset. For stock recommendations and price targets, their sample periods start in 1993 and 1999, respectively, when IBES first collects these data. As prior studies suggest that the coverage in the IBES database is thin and incomplete prior to 1983, I focus on those analysts who first appear in IBES on or after 1983 (O'Brien (1988), Zitzewitz (2001), and Bernhardt, Campello, and Kutsoati (2006)). All earnings forecasts used in the study are based on unadjusted earnings forecasts. The purpose of doing so is to avoid any potential bias documented in prior studies (e.g. Diether, Malloy, and Scherbina (2002), Payne and Thomas (2003)). Following the literature, the sample focuses on the last firm-year forecasts issued by analysts at least 30 days, but not more than one year, before the fiscal year-end. All forecasts and recommendations are merged with the Center for Research on Security Prices to obtain relevant stock prices.

Following prior studies, an analyst first starts his or her career in the first month when the analyst's first earnings forecast is recorded in IBES, and an analyst is employed in a brokerage house in month *t* if the analyst issues an earnings forecast under the brokerage house identifier. While recommendation data also contain brokerage firm identifiers, I do not use them to reconstruct the employment panel for a couple of reasons. First, the recommendation data only start in 1993, much later than the starting year of earnings forecast data. Second, analysts are more active in initiating or revising earnings forecasts than recommendations. As such, using earnings forecasts instead of recommendations tracks analysts' job separations in a timelier manner. The panel on employment history provides information on analysts and their colleagues who work in the same brokerage house in a specific timeframe.

Appendix 2
Variable Definitions

The following summarizes the definitions for the main variables used in this study.

Variable	Description/Construction Details
Accuracy	The percentile-rank accuracy following Hong, Kubik, and Solomon (2000) and Hong and Kubik (2003). This metric is calculated based on a three-year rolling window. Absolute forecast errors that are at the top and bottom 1% are excluded before ranking. This metric is further ranked each year and assigned a value between 1 (least optimistic) and 100 (most optimistic) to remove undue influence of extreme values. *Accuracy Effects* is a series of dummies that take one for each decile in *Accuracy*.
Affiliated dummy	A dummy variable that takes the value of one if an analyst works at a brokerage house that is either a lead underwriter or a co-underwriter of an initial public offering of the covered stock during the past five years or a secondary equity offering during the past two years.
All-star analyst/dummy	A dummy indicating whether an analyst is ranked as all-star analyst in the previous year's October issue of the *Institutional Investor* magazine.
All-star mentor	A dummy indicating whether at least one of the analyst's mentors has ever been ranked as an all-star analyst in the Institutional Investor magazine since 1983.
Book-to-market ratio	Ratio of book equity to market equity, where book equity is the sum of total assets, deferred taxes and convertible debt minus total liabilities and preferred stock, and market equity is the product of share price and common shares outstanding.
Brokerage house effects	A series of dummies that takes one for an increase of ten in the number of analysts who have worked in a brokerage house.
Brokerage size	The number of analysts employed by a brokerage house in the previous twelve months.
Days since last forecast	The number of days since the most recent forecast issued by any analyst following the firm.
Days to year-end	The average number of days to the forthcoming fiscal year-end. *Days to Year-End Effects* is a series of dummies that take one for an increase of 30 days before a firm's fiscal year-end.
Earnings forecast optimism	The percentile-rank optimism following Hong, Kubik, and Solomon (2000) and Hong and Kubik (2003). This metric is calculated based on a three-year rolling window. Forecast errors that are at the top and bottom 1% are excluded before ranking. This metric is further ranked each year and assigned a value between 1 (least optimistic) and 100 (most optimistic) to remove undue influence of extreme values.
Firm-specific experience	The number of years an analyst covers the firm. *Firm-Specific Experience Effects* is a series of dummies that takes one for an increase of one year of firm-specific experience of an analyst.
Firm coverage	The average coverage of an analyst's portfolio of firms. *Firm Coverage Effects* are a series of dummies that takes one for an increase of one additional analyst in an analyst's firm coverage.

Appendix 2
Variable Definitions – *Continued*

Variable	Description/Construction Details
Firm size	Natural logarithm of market capitalization of a firm in month t-1.
Forecast frequency	The number of earnings forecasts issued by an analyst in the previous twelve months.
General experience	The number of years after an analyst first appears in IBES. *General Experience Effects* is a series of dummies that takes one for an increase of one year in an analyst's experience.
Institutional ownership	Fraction of shares held by institutional managers reported in the latest quarterly 13f filings in Thomson-Reuters Institutional Holdings (13f) database divided by the number of shares outstanding.
Momentum 6m	Cumulative return in the past 6 months ending in month t-1.
Number of analysts	Number of analysts covering a given firm in the previous twelve months prior to the earnings forecast date.
Number of companies	The number of companies followed by an analyst in the previous twelve months. *Number of Companies Effects* is a series of dummies that takes one for an increase of five companies covered by an analyst.
Number of industries	The number of IBES industries followed by an analyst in the previous twelve months. *Number of Industries Effects* is a series of dummies that takes one for an increase of five industries covered by an analyst.
Optimistic mentors	A dummy that takes one when a specific mentor metric is in the top 10% or 20% in a given year. Pessimistic mentors dummy is similarly defined and takes one when a specific metric is ranked as either in the bottom 10% or 20%.
Proportion of all-star mentors	The proportion of all-star mentors ever ranked as all-star analysts in prior *Institutional Investor* magazines.
Price growth optimism	The one-year-ahead split-adjusted price target to the split-adjusted stock price on announcement date. This metric is ranked each year and assigned a value between 1 (least optimistic) and 100 (most optimistic) to remove undue influence of extreme values.
Revision magnitude	Difference between an analyst's current and previous forecast for a given firm in given year, divided by the share price two days prior to the forecast announcement date.
Revision signal	A discrete variable that takes a value of +1 (-1) when an analyst's new forecast is both above (below) his or her own prior forecast and the prior consensus for the firm, and zero otherwise.
Size-adjusted cumulative return	The size-adjusted cumulative return is the buy-and-hold return of firm j for which the revision is made minus the buy-and-hold return for an equal-weighted portfolio of firms in the same NYSE size decile formed at the beginning of each year.

Appendix 2
Variable Definitions – *Continued*

Variable	Description/Construction Details
Underwriting	The proportion of firms affiliated with an analyst's brokerage firm. A firm is affiliated with a brokerage firm if the brokerage firm is either a lead underwriter or a co-underwriter of an initial public offering of the covered stock during the past five years or a secondary equity offering during the past two years. *Underwriting Effects* is a series of dummies that takes one for each decile in *Underwriting*.
Upgrade jumps	The number of large upgrades to the total number of recommendation revisions made by an analyst in a given year. A revision is regarded as large if the revision in recommendation rating for a given firm is not to its immediately adjacent rating category. For instance, an upgrade is large when the rating is upgraded from "Sell" to "Buy." *Upgrade Jumps Dummy* is a dummy that takes one when an analyst's *Upgrade Jumps* is in the top quintile in a given year.

Appendix 3
Variable Definitions

The following summarizes the definitions for the main variables used in this study.

Variable	Description/Construction Details
Analyst Accuracy	The percentile-rank accuracy following Hong, Kubik, and Solomon (2000) and Hong and Kubik (2003). This metric is calculated based on a three-year rolling window when available. Absolute forecast errors that are at the top and bottom 1% are excluded before ranking.
Analyst optimism	The percentile-rank optimism following Hong, Kubik, and Solomon (2000) and Hong and Kubik (2003). This metric is calculated based on a three-year rolling window when available. Forecast errors that are at the top and bottom 1% are excluded before ranking.
All-star analyst dummy	A dummy indicating whether an analyst is ranked as all-star analyst in the previous year's October issue of the *Institutional Investor* magazine.
Brokerage size	The number of analysts employed by a brokerage house in the previous twelve months.
Days since last forecast	The number of days since the most recent forecast issued by any analyst following the firm.
Days to year-end	The average number of days to the forthcoming fiscal year-end.
Ever-star analyst dumm	A dummy takes one after an analyst is ranked as all-star analyst in prior issues of the *Institutional Investor* magazine.
Firm-specific experienc	The number of years an analyst covers the firm.
Firm coverage	The average coverage of an analyst's portfolio of firms.
General experience	The number of years after an analyst first appears in IBES.
Number of analysts	Number of analysts covering a given firm in the previous twelve months prior to the earnings forecast date.
Number of companies	The number of companies followed by an analyst in the previous twelve months.
Number of industries	The number of IBES industries followed by an analyst in the previous twelve months.
Recession	A dummy takes one when an analyst first appears in IBES during the years when the annual employment growth in analyst labor market is negative (i.e., during the years of 1988-91, 2002-03, or 2009-10), or zero otherwise. The employment data on *Securities, Commodity Contracts, and Investments* (NAICS: 523 series) are obtained from Bureau of Economic Analysis.
Boom	A dummy takes one when an analyst first appears in IBES during the years when the non-negative annual employment growth in analyst labor market is in its top tertile (i.e., during the years of 1984, 1986-1987, 1993-1994, 1998, or 2000), or zero otherwise. The employment data on *Securities, Commodity Contracts, and Investments* (NAICS: 523 series) are obtained from Bureau of Economic Analysis.
Underwriting	The proportion of firms affiliated with an analyst's brokerage firm. A firm is affiliated with a brokerage firm if the brokerage firm is either a lead underwriter or a co-underwriter of an initial public offering of the covered stock during the past five years or a secondary equity offering during the past two years.
Young analyst	A dummy takes one during the first three years after an analyst first appears in IBES.

Appendix 4
Robustness

The following reports the details of different robustness checks.

	Dependent Variables:									
	Forecast Optimism					*Forecast Accuracy*				
	Estimated Coefficients on *Recession*									
Descriptions	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
After 1993	-5.050	-5.444	-5.346	-4.961	-5.106	-4.363	-5.307	-5.640	-5.121	-4.117
	(-3.85)	(-4.24)	(-4.14)	(-3.86)	(-4.00)	(-3.35)	(-4.32)	(-4.53)	(-4.36)	(-3.77)
Num of obs	42,029	42,029	42,029	42,029	42,029	42,029	42,029	42,029	42,029	42,029
Adj.-R^2 (%)	0.093	2.289	2.421	6.746	7.184	0.067	3.067	3.447	11.855	22.149
After Reg FD	-6.021	-4.503	-4.442	-4.323	-4.205	-6.540	-4.936	-4.752	-3.869	-3.883
	(-3.47)	(-2.67)	(-2.64)	(-2.63)	(-2.58)	(-3.95)	(-3.15)	(-3.06)	(-2.58)	(-2.76)
Num of obs	24,156	24,156	24,156	24,156	24,156	24,156	24,156	24,156	24,156	24,156
Adj.-R^2 (%)	0.134	3.155	3.333	9.273	9.795	0.158	4.182	5.212	14.159	24.243
Left censored	-2.709	-3.477	-4.174	-4.712	-4.758	-5.382	-6.667	-6.395	-6.215	-5.503
at 1984	(-2.27)	(-2.92)	(-3.40)	(-3.83)	(-3.88)	(-4.42)	(-5.80)	(-5.39)	(-5.49)	(-5.23)
Num of obs	47,983	47,983	47,983	47,983	47,983	47,983	47,983	47,983	47,983	47,983
Adj.-R^2 (%)	0.027	1.331	1.615	5.355	5.766	0.111	2.429	2.959	10.678	20.999
At least 3 years	-8.325	-7.435	-6.568	-7.768	-4.768	-9.689	-8.221	-4.958	-4.423	-4.224
of experience	(-4.06)	(-3.68)	(-3.13)	(-3.74)	(-3.75)	(-4.83)	(-4.39)	(-2.53)	(-2.42)	(-2.47)
Num of obs	36,238	36,238	36,238	36,238	36,238	36,238	36,238	36,238	36,238	36,238
Adj.-R^2 (%)	0.149	2.305	2.661	7.084	7.553	0.205	3.830	4.768	14.410	24.473
Controlling for	-4.307	-3.012	-3.014	-3.715	3.751	-7.131	-5.450	-5.413	-5.008	-4.156
industry-year FEs	(-3.51)	(-2.49)	(-2.50)	(-3.07)	(-3.11)	(-5.70)	(-4.69)	(-4.65)	(-4.48)	(-4.01)
Num of obs	56,906	56,906	56,906	56,906	56,906	56,906	56,906	56,906	56,906	56,906
Adj.-R^2 (%)	0.263	1.790	1.787	5.475	5.848	0.169	2.713	2.961	11.341	22.021
Price-scaled	-6.375	-6.747	-1.859	-2.114	-2.192	-7.266	-7.844	-1.954	-1.618	-2.228
dependent var.	(-7.79)	(-8.32)	(-2.50)	(-2.75)	(-2.90)	(-7.38)	(-8.40)	(-2.24)	(-1.76)	(-2.59)
Num of obs	56,906	56,906	56,906	56,906	56,906	56,906	56,906	56,906	56,906	56,906
Adj.-R^2 (%)	0.139	1.469	10.193	11.857	14.029	0.167	3.677	13.828	14.866	21.949
Non-linear	-4.335	-4.391	-3.050	-3.609	-3.762	-7.006	-7.036	-5.717	-5.511	-4.335
control var.	(-3.64)	(-3.72)	(-2.53)	(-2.98)	(-3.12)	(-5.80)	(-6.23)	(-4.95)	(-4.98)	(-4.22)
Num of obs	56,906	56,906	56,906	56,906	56,906	56,906	56,906	56,906	56,906	56,906
Adj.-R^2 (%)	0.065	1.181	1.670	5.336	5.660	0.166	2.732	3.633	11.764	22.049

Appendix 4 – *Continued*

Robustness

	Dependent Variables:									
	Forecast Optimism					*Forecast Accuracy*				
	Estimated Coefficients on *Recession*									
Descriptions	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
NBER recession	2.019	0.383	-2.725	-3.012	-2.652	2.038	-2.235	-4.553	-5.694	-6.395
	(1.53)	(0.30)	(-1.99)	(-2.19)	(-1.93)	(1.48)	(-1.75)	(-3.42)	(-4.37)	(-5.28)
Num of obs	56,906	56,906	56,906	56,906	56,906	56,906	56,906	56,906	56,906	56,906
Adj.-R^2 (%)	0.011	1.312	1.744	5.388	5.782	0.011	2.491	3.049	11.402	22.033
FM regressions	-2.608	-4.037	-4.097	-4.194	-3.785	-4.916	-3.640	-3.342	-3.452	-3.450
	(-2.16)	(-3.15)	(-3.10)	(-3.31)	(-3.29)	(-1.75)	(-3.40)	(-3.32)	(-3.48)	(-3.47)
#Negative	15	20	19	18	18	16	17	17	18	19
#Negative+sig.	5	7	6	7	4	11	7	6	5	7
#Positive	8	3	4	5	5	7	6	6	5	4
#Positive+sig.	1	0	0	0	0	4	1	1	1	0
Num of obs	56,906	56,906	56,906	56,906	56,906	56,906	56,906	56,906	56,906	56,906
Adj.-R^2 (%)	0.111	1.642	1.618	6.939	7.858	0.493	3.154	3.617	9.369	21.344

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